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THE BOGOSLOFS

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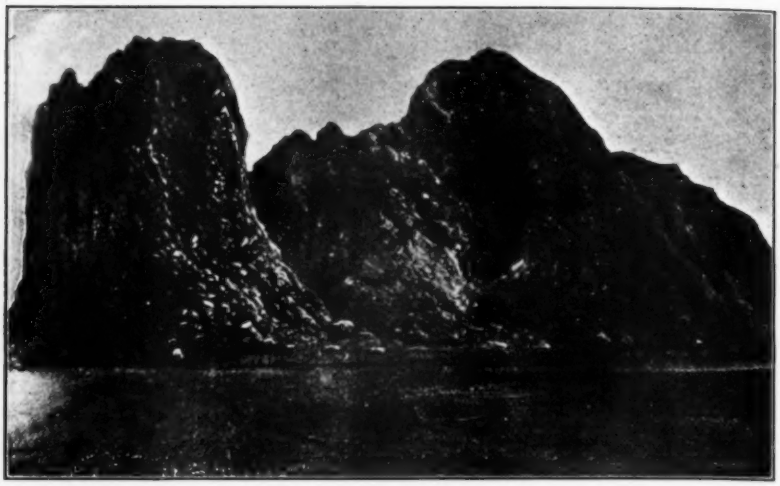
IN the southern portion of Bering Sea, about thirty-seven nautical miles northwest from the island of Unalaska, lies a group of small volcanic islets known as Bogoslof, in Russian, Joanna Bogoslova, St. John, the Theologian. There are now three of these, all of which have risen from the sea, hot and steaming, within historic times. An especial interest attaches to them just now from the fact that the third and largest of the group appeared at about the time of the great earthquake of April 18, 1906.

The possibility of a connection between the disturbances at Bogoslof and those which caused the California earthquake is heightened by the fact that the great earthquake rift, which extends through the Coast Range of California for a distance of 200 miles, follows a direction, which, if produced northward to Bering Sea, would pass near the islands of Bogoslof. Again this earthquake rift was largest, and its effects more violent, where it entered the sea in Mendocino County than at any other point throughout its course, the extent of the lateral movement along the crack increasing from about two feet in Monterey County to about 16½ feet at Point Arena, where it finally enters the sea.

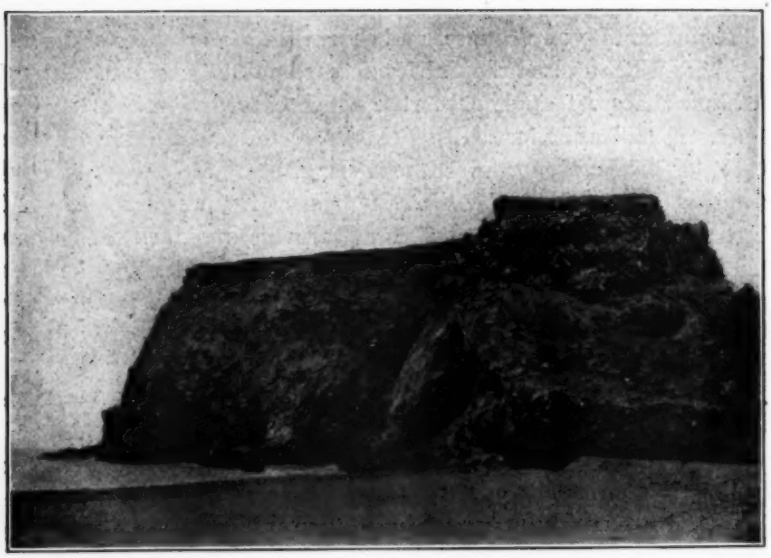
In opposition to this view may be placed the improbability that an earthquake rift or fault would extend so far as from the center of California to Bering Sea, a distance of more than 2,000 miles, and through such great depths of water as intervene between Point Arena and Bogoslof. It is also stated that the evidence of the seismograph, so far as understood, favors the idea that the great earthquake was



OLD BOGOSLOF OR CASTLE ISLAND.

confined to California, although its center of disturbance was clearly in the sea in a westerly direction from Cape Mendocino.

It is evident also that the rise of the third Bogoslof was attended by little if any disturbance in the immediate vicinity. The advent of each of the other two islands was marked by earthquake shocks, the fall of volcanic ashes and displays of fire, observed and felt by the people of Iliuliuk on Unalaska Island. The people of this village in 1906 were unaware of the presence of the new island until the news was brought



FIRE ISLAND, ONE OF THE OLD BOGOSLOF ISLANDS.

in by vessels touching at the harbor. Earthquake shocks lasting 30 seconds are reported for May 20 and 23 by the keeper of the light at Scotch Cap on Unimak Island, and a 'pretty severe shake' occurred at Dutch Harbor on June 2, but nothing is reported for April or early May, when the new island must have risen. Certainly there could not have been any activity displayed by Makushin or Akutan, both of which volcanoes overlook Unalaska and Dutch Harbor, without being observed by the people of these villages. Perhaps the rise of such an island, in a more or less plastic condition, as it must be, would not necessarily be attended by disturbance in the solid crust of the neighboring islands. On the Pribilof Islands, which had an origin similar to that of the Bogoslofs, no earthquake shock or other disturbance was noted, although

FIRE ISLAND, ONE OF THE OLD BOGOSLOF ISLANDS.

these islands were affected at the time of the rise of New Bogoslof in 1883. The Pribilof group lie 120 miles to the north of the Bogoslofs.

On the whole, however, the weight of evidence at present seems to favor the idea that the Bogoslof disturbance of 1906 was local in character and the coincidence in date with the California earthquake involves no actual relation between the two phenomena.

The writers first saw the islands of Bogoslof in July, 1896, while en route for the Pribilof Islands in connection with the fur seal investigations. The U. S. Fish Commission Steamer *Albatross* attempted to land the commission on Old Bogoslof, but was prevented by the heavy surf, and the thick weather made only a partial view of the islands possible. The vessel afterwards passed the islands on its way to the

NEW BOGOSLOF, OR FIRE ISLAND (1883).

THE NEW BOGOSLOF ISLAND

Commander Islands under more favorable conditions. Dr. Stejneger of the commission obtained some excellent photographs. The senior writer, still later in the same season, passed both islands while on the British gunboat, *Satellite,* on the way from the Russian Islands to Unalaska.

At that time Old Bogoslof, known to the sealers as Castle Island, from its appearance, was cold and dead. It showed in the fog a sheer cliff or hill of ashes about 300 or 400 feet in height, seeming much higher in the uncertain light. It was apparently the home of countless sea birds and a small herd of the gray sea lions (*Eumetopias stelleri*) was hauled out upon one of its slopes.

About half a mile to the northwest lay the islet of New Bogoslof, of about twice the height of the other and considerably greater area. This island was locally known as Fire Island, having but recently ceased to steam and smoke. There was in 1896 no evidence of activity in it, but the water was said to be still warm in the crevices of the rocks. The name Grewingk, in honor of the Russian geographer who compiled an early account of Old Bogoslof, has been given to this island by Mr. Dall.

Both islands were surrounded with deep water. In fact the space occupied by the second island had formerly been safely traversed by vessels. Dredge hauls by the *Albatross* about the islands resulted in the taking of a number of deep sea forms of fishes, among them three 'grenadiers' (*Albatrossia pectoralis, Bogoslovius clarki,* and *Macrourus cinereus*). These were obtained at a depth of 664 fathoms or 3,984 feet.

Conspicuous in the group of islets was an isolated pillar of rock, of considerable height, known as Ship or Sail Rock. It had existed from the earliest times, having been reported as early as 1768. It was seen by Captain Cook in 1778, who mistook it for a ship under sail, hence its name. This was eighteen years before the rise of Old Bogoslof. Ship Rock crumbled and fell in ruins about 1888.

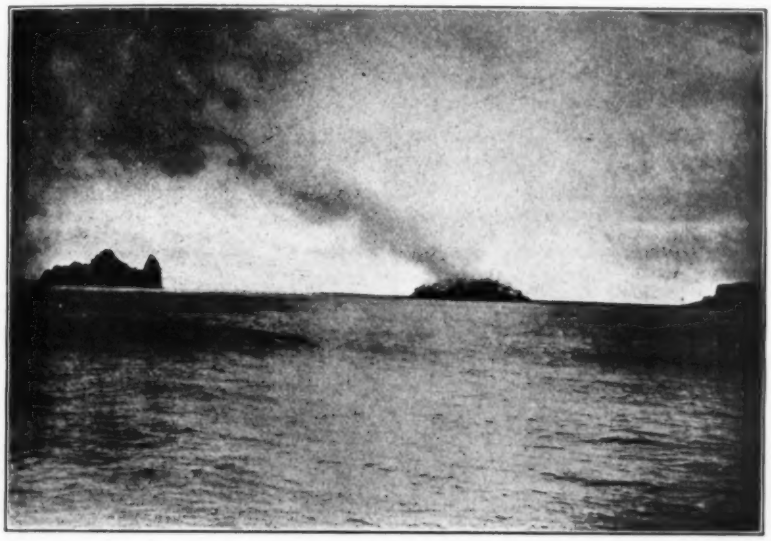
About April, 1906, midway between Old and New Bogoslof, a third island, larger than either of the others appeared. Captain Dirks of Dutch Harbor estimates its size as five times that of New Bogoslof, although the photographs do not seem to bear this out. This new island was first seen by the U. S. Fish Commission Steamer *Albatross,* Captain L. M. Garrett, on May 28, 1906, while on her way to the investigation, under direction of Professor Charles H. Gilbert, of the fisheries of Japan. Soon after this date the U. S. Revenue Cutter *Perry* visited the islands. Photographs were taken under the direction of Lieutenant Hepburn, of the *Albatross,* and these, sent us by Mr. H. H. Taylor, of the North American Commercial Company, are here reproduced, together with photographs of Castle Island and Fire Island, taken by Mr. N. B. Miller of the *Albatross* in 1892.

The early history of these very interesting islands is given by Professor George Davidson in the *Bulletin of the American Geological Society,* Vol. XXII., p. 267, and a detailed and exhaustive account of them by Dr. C. Hart Merriam, profusely illustrated, appears in the Report of the Harriman Expedition of 1899, Vol. II., p. 291–336.

BOGOSLOF OF MAY, 1906. FROM NEW BOGOSLOF, OR FIRE ISLAND.

Of the advent of the first island, in 1796, the following account is given in Kotzebue's narrative of discovery in 1817. The story is that of a Russian trader, Kriukof, who found himself with some native hunters forced to seek refuge from storm on the north end of Umnak Island, the island of the Aleutian chain, nearest the Bogoslofs. It was in May and when the storm cleared on the 8th, Kotzebue tells us:

They saw to the N., several miles from land, a column of smoke ascending from the sea; toward evening they observed under the smoke something black, which arose but a little above the surface of the water. During the night fire ascended into the air near the spot, and sometimes so violent, and to such height, that on their island, which was ten miles distant, everything could be distinctly seen by its light. An earthquake shook their island, and a frightful noise echoed from the mountains in the S. The poor hunters were in deadly anxiety; the rising island threw stones towards them, and they every moment expected to perish. At the rising of the sun the quaking ceased, the fire visibly decreased, and they now plainly saw an island of the form of a pointed black



THE THREE BOGOSLOFS, MAY, 1906.

cap. When Kriukof visited the island of Oomnak, a month afterward, he found the new island, which during that time had continued to emit fire, considerably higher. After that time it threw out less fire, but more smoke: it had increased in height and circumference, and often changed its form. For four years no more smoke was seen, and in the eighth year the hunters resolved to visit it. as they observed that many sea lions resorted to it. The water round the island was found warm, and the island itself so hot in many places that they could not tread on it.

The eruption of 1883, which resulted in the rise of New Bogoslof, seems to have had no eye-witnesses and the exact date of its appearance is unknown. Captain Anderson of the schooner *Matthew Turner*

saw the new island in September, 1883, and reported that great volumes of steam and smoke, accompanied by showers of ashes, were thrown out from the summit and through fissures in the sides and base, the bright reflections from the heated interior being visible at night. At the time of this eruption a severe earthquake was felt in the sea off Cape Mendocino, apparently in the line of the Portolá-Tomales rift of April, 1906.

The islands were visited in 1884 by the officers of the U. S. Revenue Cutter *Corwin,* and Lieutenant J. C. Cantwell and Surgeon H. W. Yemans made the ascent of New Bogoslof. Lieutenant Cantwell thus describes his experience in the 'Cruise of the *Corwin*':

The sides of New Bogoslof rise with a gentle slope to the crater. The ascent at first appears easy, but a thin layer of ashes, formed into a crust by the action of rain and moisture, is not strong enough to sustain a man's weight. At every step my feet crushed through the outer covering and I sank at first ankle-deep and later on knee-deep into a soft, almost impalpable dust which arose in clouds and nearly suffocated me. As the summit was reached the heat of the ashes became unbearable, and I was forced to continue the ascent by picking my way over rocks whose surfaces, being exposed to the air, were somewhat cooled and afforded a more secure foothold.

On all sides of the cone there are openings through which steam escaped with more or less energy. I observed from some vents the steam was emitted at regular intervals, while from others it issued with no intermission. Around each vent there was a thick deposit of sulphur which gave off suffocating vapors.

The islands were visited by Drs. C. Hart Merriam and T. C. Mendenhall of the Bering Sea Fur Seal Commission in 1891. Dr. Merriam writes thus of New Bogoslof as seen at that time:

The new volcano was enveloped in steam, which issued from thousands of small cracks and crannies and poured in vast clouds from a few great fissures and crater-like openings, the principal of which was near the northwest corner, only a few feet above high water mark. From this opening, the shape of which we could not see, it rushed out with a loud roaring noise. So great was the quantity of steam that it completely concealed the upper part of the island except when wafted to and fro by violent gusts of wind. . . . The steam was usually impregnated with fumes of sulphur, and deposits of sulphur, some in very fine needles, were observed along the margins of the cracks.

Of the third Bogoslof, Dr. Charles H. Gilbert, of Stanford University, who was in charge of the work of the *Albatross* when the 'brand new mountain' was first seen on May 28, 1906, writes thus in a personal letter regarding it:

When I saw it (Bogoslof) in 1890 there were really two small islands about 1½ miles apart, one of them steaming and the other already cooled off. This has been the condition for a number of years, so the hot one had received the name of Fire Island, the cold one, Castle Island. When they came in sight yesterday, we were astonished to find that Fire Island was no longer smoking and that a very large third island had arisen half way between the other two. It was made of jagged, rugged lava and was giving off clouds of steam and smoke

from any number of little craters scattered all over it. Around these craters, the rocks were all crusted with yellow sulphur.

In a later letter, written from Yokohama, Dr. Gilbert said:

I wrote you a full account of Bogoslof, but the letter seems to have miscarried. Our discovery seems to have been corroborated later by some revenue cutter, but if the newspaper report agrees with their findings, very extensive changes took place in the interval between the two visits. When seen by us, the new cone, occupying much of the space between the two older ones, was somewhat higher than either, but was certainly far from 900 feet high—300 feet would be an extreme figure. There was no evidence of a central crater. The steam and fumes were given off most abundantly from cracks and fumaroles on the slopes. About these were heavy incrustations of sulphur. We saw no indications of boiling water, nor did we believe that landing would be impossible.

In an account of the physical history of the Bogoslofs, written in 1899 for the report of the Harriman Expedition, Dr. Grove K. Gilbert, of the U. S. Geological Survey, noting the rapid disintegration of the islands, said:

One might predict that in the next century the name Bogoslof would attach only to a reef or shoal, were it not for the possibility of new eruptions. The pulse of the volcano is so slow that we have noted only two beats in more than a century, but such sluggishness must not be taken as a symptom of death, or even decline, for volcanic organisms are characteristically spasmodic in their activity. Long before the sea has established its perfect sway the arteries of the mountain may again be opened and a new and larger island put forth to contest its supremacy.

Nearly a century elapsed between the arrival of the first and second Bogoslof, only twenty-three years between the second and third.

The floor of the depths of Bering Sea in this region seems to be still unsettled, and astonishing changes may be looked for at any time. If it should prove true that the geological faults of California extend out from this center, a new interest would be attached to the outbreaks of Bogoslof.

NOTES ON THE DEVELOPMENT OF TELEPHONE SERVICE. II.

By FRED De LAND
PITTSBURG, PA.

IV. *Exploiting The Telephone*

IN the fall of 1876, Gardiner Greene Hubbard began to systematically exploit the electric-speaking telephone invented by Alexander Graham Bell. In the vicinity of Boston a number of private telephone lines were strung, some of which were two or three miles in length, to connect mills and offices or offices and residences. In some instances, where private telegraph lines already existed, the telegraph instruments were replaced with a pair of telephones.

On October 9, 1876, a telephone was attached to each end of a telegraph circuit owned and operated by "The Walworth Manufacturing Company, extending from their office in Boston to their factory in Cambridge, a distance of about two miles. The company's telegraph battery consisting of nine Daniell's cells, were removed from the circuit, and another of ten carbon elements was substituted." It is recorded that "articulate transmission then took place through the wire. The sounds, at first faint and indistinct, became suddenly quite loud and intelligible." Another instance of the early practical use of the telephone was in connecting the water works with the central office of the water commissioners, of Cambridge, Mass. On April 4, 1877, a telephone circuit was strung to connect the factory of Charles Williams, Jr., in Court Street, Boston, with his residence in Somerville. This is said to be the first telephone circuit constructed in the United States, the earlier ones being transformed telegraph lines. A number of other private telephone lines were built in and about Boston early in 1877. In fact a number of small contractors found it profitable to string private lines, and strove to secure orders for this class of work. For they would run the circuits on the poles of the telegraph companies without permission, or bracket them to house-tops, to trees, to any place that a bracket or a porcelain knob could be attached, paying no attention to property rights.

In the winter of 1876–77, experimental toll service over telegraph circuits was successful for distances of several hundred miles, even from Boston to New York. In November, 1876, Graham Bell found no difficulty in carrying on conversation over telegraph circuits between New York and Boston, using only a pair of box magneto-telephones, so long as the parallel wires were not in service. "When this hap-

pened," he said, "the vocal sounds were very much diminished, but still audible. It seemed, indeed, like talking in a storm. Conversation, though audible, could be carried on with difficulty."

On February 12, 1877, Graham Bell delivered a lecture on the telephone at Salem, Mass., eighteen miles from Boston. At 10:55 P.M., a reporter of the Boston *Globe* turned in his report by telephone and this was the first newspaper report sent by telephone. Previous to the lecture a wire was strung from the lecture hall to a telegraph circuit connected with the editorial rooms. Then a single telephone was placed on each end of the line.

The telephones used in these pioneer lines were of the box magneto type and intended to rest on shelf, table or desk. Several modifications of this form were made. An interior view of the form sent out in April, 1877, is shown in Fig. 4. Then came the wooden hand telephone (Fig. 5) in May, 1877. Owing to its resemblance to a well-known kitchen utensil, it was promptly called 'the butter-stamp telephone.' It was such a decided improvement in shape and convenience over the box telephone that it won its way from the start. Then came the first of the electric-speaking telephone circulars. Three pages contained endorsements by the press and scientists, while the first page contained the following statements that appear unique in the light of later knowledge:

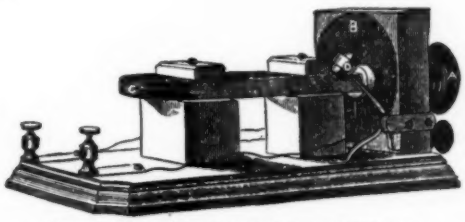


FIG. 4. BOX TELEPHONE, COVER REMOVED.

The Telephone

The proprietors of the telephone, the invention of Alexander Graham Bell, for which patents have been issued by the United States and Great Britain, are now prepared to furnish telephones for the transmission of articulate speech through instruments not more than twenty miles apart. Conversation can be easily carried on after slight practice and with occasional repetition of a word or sentence. On first listening to the telephone, though the sound is perfectly audible, the articulation seems to be indistinct; but after a few trials the ear becomes accustomed to the peculiar sound and finds little difficulty in understanding the words.

The telephone should be set in a quiet place, where there is no noise which would interrupt ordinary conversations.

The advantages of the telephone over the telegraph for local business are

1. That no skilled operator is required, but direct communication may be had by speech without the intervention of a third person.

2. That the communication is much more rapid, the average number of words transmitted a minute by Morse sounder being from fifteen to twenty, by telephone from one to two hundred.

3. That no expense is required either for its operation, maintenance, or repair. It needs no battery, and has no complicated machinery. It is unsurpassed for economy and simplicity.

The terms for leasing two telephones for social purposes connecting a dwelling-house with any other building will be $20 a year, for business pur-

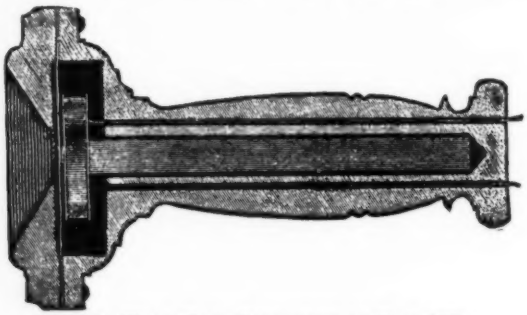


FIG. 5. WOOD HAND TELEPHONE OF MAY, 1877.

poses $40 a year, payable semiannually in advance, with the cost of expressage from Boston, New York, Cincinnati, Chicago, St. Louis, or San Francisco. The instruments will be kept in good working order by the lessors, free of expense, except from injuries resulting from great carelessness.

Several telephones can be placed on the same line at an additional rental of $10 for each instrument; but the use of more than two on the same line where privacy is required is not advised. Any person within ordinary hearing distance can hear the voice calling through the telephone. If a louder call is required one can be furnished for $5.

Telegraph lines will be constructed by the proprietors if desired. The price will vary from $100 to $150 a mile; any good mechanic can construct a line; No. 9 wire costs 8½ cents a pound, 320 pounds to the mile; 34 insulators at 25 cents each; the price of poles and setting varies in every locality; stringing wire $5 per mile; sundries $10 per mile.

Parties leasing the telephone incur no expense beyond the annual rental and the repair of the line wire. On the following pages are extracts from the press and other sources relating to the telephone.

Cambridge, Mass., May, 1877. GARDINER G. HUBBARD.

For further information and orders adress

THOMAS A. WATSON, 109 Court St., Boston.

The work of supplying to customers the hand telephones, referred to in the foregoing circular, was entrusted to Graham Bell's assistant, Mr. Thomas A. Watson, who had entered the employ of the proprietors of the telephone about April 1, 1876. He occupied a small amount of desk room and much bench room in the small factory of Charles Williams, at 109 Court Street, Boston. Here Mr. Watson made up and assembled the parts, as the telephones were called for. Naturally, improvements were the order of the day, and soon a smaller and more attractive mahogany handle magneto-telephone was adopted.

How rapidly 'Bell's toy' began to win its way into public favor is indicated by the statement that on July 31, 1877, or less than four months from the day the first circular was sent out by Mr. Hubbard, 778 telephones had been leased, while in all probability an equal number of experimental telephones had been made by mechanics and scientists who thought that it would be an easy matter to improve upon Bell's method. When the year 1877 closed, there were 5,491 Bell telephones in use.

Naturally this good demand for so serviceable an instrument encouraged 'the proprietors' of the Bell patents to branch out on a little broader basis. As Graham Bell had transferred all right, title and interest to Mr. Hubbard on July 9, 1877, thus placing the control in the latter's hands, on August 1, 1877, Mr. Hubbard organized the Bell Telephone Association, of Boston, without capital stock and served as trustee, while Mr. Sanders acted as treasurer.

Then the development of the exchange business and the assignment of territorial rights began in earnest, and Mr. Hubbard visited all the larger cities seeking to interest men of prominence. But though he journeyed hither and thither, striving to influence capital to favorably consider the telephone as a desirable investment, yet the task of interesting investors in the development of local exchanges proved difficult, and progress was made slowly in the United States.

In Europe some progress was made through elaborate experiments carried on by foreign governments to practically demonstrate the utility of the telephone. On November 28, 1877, it was officially promulgated that 'the introduction of the telephone in the practical telegraph service of the German Empire has been formally accomplished; and the passing of the telephone into practical use may be regarded as satisfactorily completed.' This conclusion was based largely on the excellent results secured on a toll circuit two hundred and thirty miles long, established between Berlin and Prince Bismarck's country residence at Varzin, in Pomerania, early in October, 1877. Probably that was the first official recognition of the practical value of the telephone on the part of a foreign nation. Yet, early in 1877, Mr. Preece, the head of England's telegraph department, had notified his government that Alexander Graham Bell "has rendered it possible to reproduce the human voice with all its modulations at distant points. I have spoken with a person at various distances up to thirty-two miles." In November, 1877, conversation was excellently maintained for two hours between Dover and Calais, a distance of twenty-two miles, by using a telegraph circuit in a submarine cable. Then, in December, 1877, Mr. Preece officially reported having successfully carried on a long conversation through a submarine telegraph cable sixty-seven miles in length, extending from Dublin to Holyhead, by means of hand telephones.

In the United States the first lease for territorial rights was executed on October 24, 1877, with the Telephone and Telegraph Construction Company, of Detroit, Michigan; yet eleven months passed before a telephone exchange was opened in that city.

The second lease was assigned to the District Telephone Company of New Haven, Connecticut, and included the counties of New Haven and Middlesex. The former county was rapidly developed and has the honor of having established within its limits the first two commercial telephone exchanges (at New Haven and at Meriden), the first mutual

telephone exchange (Bridgeport), the first private branch exchange system (Ansonia), and the first telephone toll lines regularly connected to operating commercial telephone exchanges.

On February 12, 1878, territory was assigned to the New England Telephone Company, of Boston. On March 8, 1878, a license was granted to the American District Telegraph Company, of St. Louis, and on July 3, 1878, a license was granted for New York City and including a radius of thirty-three miles.

This activity created quite a demand for telephones, and by the end of July, 1878, over 12,000 had been placed. Thus it was deemed wiser to organize a larger and more flexible corporation. So, on July 30, 1878, Mr. Hubbard organized the Bell Telephone Company, of Boston, under the laws of Massachusetts, to manufacture, sell and use telephones outside of New England, and capitalized it at $450,000. Mr. Hubbard was trustee, Mr. Sanders, treasurer, Dr. Bell, electrician, and Thomas A. Watson, superintendent. This company acquired all the patents, property and rights of its predecessor, the Bell Telephone Association, and its headquarters remained at the Williams factory at 109 Court Street.

This second parent company entered into nine formal agreements granting exclusive rights to use Bell telephones in certain specified territory, and did such excellent work in developing the opening of exchanges, that some 22,000 telephones were in service when, on March 13, 1879, it was deemed the wiser plan to organize a new parent company having sufficient financial backing combined with the personal and commercial influence that would enable the business to be handled on the large scale that ripening conditions demanded. So the National Bell Telephone Company was chartered under the laws of the state of Massachusetts, and the company capitalized at $850,000. The executive offices of this parent company were moved to New York, in August, 1878, and located at 66 and 68 Reade Street. A year later the company moved back to Boston and secured offices at 95 Milk Street.

The officials and directors of the National Company were:

W. H. Forbes, President.
G. L. Bradley, V.-Pres. and Treasurer.
T. N. Vail, General Manager.
Alexander Graham Bell, Electrician.
Francis Blake, Jr., Electrician.
O. E. Madden, Supt. of Agencies.
Thomas A. Watson, General Inspector.

Directors

William H. Forbes, Francis Blake, Jr., Charles Eustis Hubbard,
Charles S. Bradley, Richard S. Fay, Alexander Cochrane,
Gardiner G. Hubbard, George Z. Silsbee, George L. Bradley.
Thomas Sanders, William G. Saltonstall,

Executive Committee

W. H. Forbes, Gardiner G. Hubbard, Francis Blake, Jr.
R. S. Fay, Thomas Sanders,

Now to turn back a year. Before the close of 1878, men successful in other lines of industry perceived that if the crude and limited

facilities offered by these early exchanges afforded a service esteemed by the public as a desirable convenience, then the manifold advantages that might be derived from a telephone exchange system embracing all the probable users of telephone service in a town or city, would be in direct ratio to the growth and expansion of that system. Hence, as it appeared evident that this new industry had come to stay, and was quite likely to prove a good revenue producer, capitalists began to look favorably upon 'Bell's toy,' to wonder whether it might not turn out to be an exceedingly valuable invention, and whether it was safe to infringe the Bell patents. Thus it came about that before the close of 1878, a number of promoters who had formerly scoffed at the inventor and his telephone were offering large sums in cash for exclusive rights to operate in given territory, in several cases paying a good premium for the same rights offered for a nominal payment a year previous.

These sudden conversions to implicit belief in the tangible value of the telephone appear the more remarkable when we recall the fact that throughout the United States commercial and financial affairs remained in a depressed condition during the entire year of 1878, and it was an exceedingly difficult matter to get capital interested in any new enterprise. Nevertheless, nearly seventy Bell exchanges were in process of being planned, or were under construction, or were in operation when the year closed. In the following named cities Bell exchanges were in operation at the close of 1878, and, while the number of telephones shown in service is comparatively small, the records show that several of these exchanges had secured from two to four times that number of contracts, and were connecting new subscribers as rapidly as possible.

Albany	250	Indianapolis	150
Baltimore	100	Lowell	200
Boston	150	Meriden	100
Bridgeport	175	New Haven	350
Buffalo	250	New York	250
Chicago	550	Philadelphia	250
Cincinnati	200	St. Louis	325
Columbus	50	Toledo	100
Detroit	150	Troy	100

Then exchanges were in process of construction in Washington, Louisville, New Orleans, Nashville, Cleveland, Springfield, Hartford, Providence and other places.

A very different condition of affairs prevailed in financial and commercial circles a year later, when, in the autumn of 1879, the resumption of specie payment caused a feeling of elation to pervade all branches of industry, and started a remarkable boom in railway construction and in stock speculation that spread throughout the country. Thus it was not surprising that many investors appeared eager to identify themselves with the telephone industry, nor was it so remarkable that as one of the results of this frenzied activity, there were several hundred operating Bell companies when the year of 1879 closed.

Each of these companies operated under its own management, was governed by its own policy, and supplied its service at such rates as each thought best adapted to meet the views of local patrons. There was little or no uniformity in these rates, for the majority had been established not only without regard to the brief but costly experience with low rates that the companies established in 1878 had passed through, but adversely to the sensible suggestions of the parent company to make the service so good that business houses would pay at least a dollar a week for local telephone service. Again, not only were rates established without a due regard for the amount of cash investment that would be required per subscriber, but in entire forgetfulness of two essential factors in determining cost of production and supply: (1) Decrease in plant valuation due to improvements in the art, and (2) the destructive action of the elements.

In some states there was, in 1879, a Bell license for each county, and as each licensee was wholly independent of any and all other licensees, there naturally came to be a great diversity of opinions regarding proper methods of construction and operation, equitable interchange of toll-line traffic, profitable rates and the legal protection that the parent company should insure to its licensees. Furthermore, the broader-minded licensees began to perceive that the telephone business, instead of being merely a local issue, was not only interurban and interstate in character, but continental in scope, and that the healthy growth and ultimate success of these operating companies were largely dependent on the scope and the character of the service supplied, rather than on patent protection. In 1879, it was also foreseen that an amount of capital many times larger than the original estimate called for would have to be invested to place the business on a permanent foundation. Thus the wisdom of consolidating these small county licensees into large state or interstate companies was perceived, and large operating companies controlling exchanges in many counties were in existence before the close of 1879.

Incidentally it is worth while to recall that while some of the pioneers were men to whom too much credit can not be given for the intelligent and persistent manner in which improvements in and extensions to the service were introduced, there were other pioneers whose grasp of the problems they were facing was exceedingly slight, though these latter gentlemen had no hesitation in branding as heretical all views opposed to their own, or in combatting the progressive suggestions of the parent company. Even the technical press was pessimistic in belief concerning the future of the telephone. In 1882, the editor of *The Operator* wrote: "The telephone is almost entirely a local convenience, nearly as much so as gas lighting and horse cars; its monopoly, which is not an oppressive one, rests upon the possession of patents, and must expire with the patents."

That some among the principal Bell licensees had much to learn concerning telephone problems is well illustrated by the published statement of the president of one of the most prominent companies. In 1883, he was quoted concerning the need of long-distance service, as follows:

> In the first place it has never been demonstrated that a continuous wire of one hundred miles is necessary. Such a wire has never been called for or demanded for any purpose, financial or otherwise. How could it pay? You may accept as a fact that no system of telephony which extends beyond a radius of twenty or thirty miles from any city, however large, will prove a paying investment. For purely commercial or financial interests, the telephone will not come into favor as a medium of direct transmission between large cities. The telephone, like any other enterprise, is valuable so long as it pays, and when it ceases to prove profitable in a pecuniary point of view, its scientific uses will not avail much. To sum up the whole matter, the value of the telephone is confined, as I have mentioned, within a certain radius—that is where it has many lines, and beyond that it has no money earning-capacity.

Yet within a year from the publication of that statement a number of toll lines, each more than a hundred miles in length, were in operation, notably one from Denver to Pueblo, in Colorado, one hundred and eleven miles long. This line was built with only 2,619 poles, and cost only about $13,000. Under present methods of construction a line of corresponding length and built in that section of country would probably include 4,900 poles and would cost about $90,000, including a heavy copper circuit.

Another apt illustration is found in the interview given in 1883, by the president of two large Bell companies, and who was called 'the leading practical expert of the country.'

> To talk over the 1,200 miles between Chicago and New York there must be used either a compound wire or an iron one several times as heavy—an impracticable size. The copper wire would require about 36 poles to the mile, and I have roughly estimated its cost at $400,000. Only one wire could be used upon one set of poles, for even at the extreme ends of long cross arms, at such a distance what was said to one wire would be heard on the other and *vice versa*, owing to induction. In fact we find it impracticable for this reason to put more than one wire on one set of poles for distances greater than three miles—four miles being the very limit, even if far apart.

V. *The First Mutual Telephone Exchange*

On Friday evening, April 27, 1877, twenty-nine years ago, Alexander Graham Bell delivered a lecture on his electric-speaking telephone at the opera house in New Haven, Connecticut, and also addressed audiences in Hartford and in Middletown, with the aid of telephones connected to a telegraph circuit loaned by the Atlantic and Pacific Telegraph Company. Mr. Frederick Gower delivered the lecture in Hartford, and Mr. Thomas A. Watson was in charge in Middletown. After giving a number of interesting illustrations of the serviceability of the telephone, and the ease with which conversation could be carried on over considerable distances, Dr. Bell claimed that the time was coming when a telephone in every house would be considered

indispensable, that the telephone would displace the telegraph in many business transactions, and that a man of business would have no more difficulty in talking with his agent a hundred miles away than in directing his servant through the speaking-tube. And he further stated that the telephone wires would yet be laid underground, as gas and water pipes are now laid. In Hartford, Mr. Gower explained the telephone and the many ways in which it could be utilized, and said:

> And Smith will have a wire to the central office in Hartford, while his correspondent or branch house in Boston or New York will be similarly connected there. Smith will say to the hole in the wall: Switch me upon 500 State street, Boston. Whereupon the central officer will turn the little lever, and Smith may talk with his friend all day.

Back to that lecture in April, 1877, more than twenty-eight years ago, dates the conception of the movement that resulted in the establishment of the modern telephone exchange. For the earliest among all telephone exchanges were established in Connecticut, Bridgeport claiming the honor of the first mutual telephone exchange system, and Hartford the second; Ansonia, the first private exchange system in which a regular switching system and operator were employed; to New Haven rightfully belongs the honor of having the first commercial telephone exchange ever opened, while to Meriden is credited the second of the commercial exchanges. And the manner in which the telephone was introduced for public use in each place is indicative of the way in which it was first established in many other cities.

During the past twenty years Mr. Thomas B. Doolittle has been one of the most widely-known, capable and companionable telephone men in the country, and has planned a greater mileage of telephone pole lines than any other man. In 1887 Mr. E. J. Hall said: "The first really practical success in talking over long distances was the copper metallic circuit constructed between New York and Boston by Mr. T. B. Doolittle, for the American Bell Telephone Company, in 1883. The distance was about 300 miles, and I call it the first practical success because it was the first circuit that worked at all times regardless of outside electrical disturbances."

In 1874 Mr. Doolittle was a well-known manufacturer of metal goods in Bridgeport, Connecticut. During that year he assisted in the establishment of a social telegraph system, in which some twenty or thirty users of the Morse code were connected by telegraph circuits that terminated in a special switchboard in the Bridgeport office of the Atlantic and Pacific Telegraph Company, the necessary switching being performed by the local telegraph operator.

In June, 1877, the closing of the A. & P. telegraph office, through absorption by the Western Union, temporarily suspended this local service, and necessitated other arrangements. Having become a firm believer in the future of the telephone, Mr. Doolittle secured four pairs of Bell's wooden hand telephones, and placed one pair on each of four

lines; then he installed a home-made switching device in his office, and for a signaling device employed a single-stroke bell operating on a gravity-battery current. Then he devised a method of preventing cavesdropping that in May, 1878, was declared 'a great improvement on any other telephone system now in use.' So satisfactory did these instruments prove that other lines were similarly equipped and soon a mutual telephone exchange system was in full operation, that later on formed the nucleus of the commercial exchange opened in Bridgeport by Mr. Doolittle in 1878.

In the fall of 1877 he planned and built a private telephone exchange system for the Ansonia Brass and Copper Company, of Ansonia, that, while in no sense a commercial exchange system, was indirectly of incalculable benefit to the growth and prosperity of the entire commercial telephone industry. For in planning this system Mr. Doolittle decided to use circuits of copper instead of iron, and, after many experiments, produced a hard drawn copper wire of his own adaptation, the drawing of which he supervised in the mills of that company.

Prior to that time it had been found impossible to use copper wire on pole line circuits, as its extreme ductility proved a source of continued elongation in all spans of any length and where the strain was constant. By the Doolittle process the tensile strength of the wire was greatly increased, its elongation reduced to about one per cent., and there was no appreciable change in its conductivity. Yet it required ten years of costly experience with iron wire circuits before the telephone interests fully comprehended the inestimable value of this improvement. In September, 1880, at a conference of telephone men, the representative of a very large wire-drawing mill stated that "copper wire has ceased to be discussed for telephone line use. It is too soft and elongates too readily under exposure. The suitable wire must be tough pure iron, well manipulated to secure flexibility and toughness."

Referring to this pioneer work, Mr. Doolittle wrote:

Hard drawn copper was the result of an adaptation rather than a discovery, although many of its valuable properties were not appreciated until after it had been in service several years. Prior to its introduction for aerial conductors there was very little, if any, call for the hard product. Copper wire was usually annealed after drawing and sold in that form. Copper alloyed with other metal was, and is now, used in the manufacture of hard or spring wire. It is the common knowledge of all who are familiar with the manipulation of copper that the process of drawing it into wire serves to harden the surface. Thus it will be seen that the experiments which resulted in the so-called hard drawn copper wire were based upon a well-known principle, although the application of this principle had never been made use of for the final product. The writer was familiar with this phenomenon of the hardening of drawn copper at the time he entered the field of electricity; therefore, when it was disclosed to him that copper was not only one of the best conductors of electricity, but was the cheapest in conductivity, or per mile ohm, it was only left for him to determine whether or not this hardening process could be made available in order that copper wire

should be comparable to iron in its ability to stand the strain of its own weight when strung on poles, and, in addition thereto, the weight of sleet or snow and wind pressure. There was no mathematical road to determine this factor; therefore it was simply a case of 'cut and try.'

It was for producing in 1877 the five hundred pounds of No. 12 B. & S. gauge hard drawn copper wire used in stringing the aerial circuits in the Ansonia Brass and Copper Company's private telephone exchange system that the Franklin Institute awarded to Mr. Doolittle the Edward Longstreth medal of merit.

In connecting up the different offices and mills of the Ansonia Brass and Copper Company he built substantial pole lines, installed a home-made type of switchboard to which all circuits were attached and had a regular operator employed. A pole line was also built to the freight station of the Derby Railroad Company, and a telephone installed there and connected to the main switchboard, thus enabling any department to get in immediate communication with the freight station. This system was completed and in successful operation on December 4, 1877. Then, when the Connecticut Telephone Company came into possession of the territory by virtue of purchasing the rights of earlier Bell licensee systems, and thus possessed the sole right to operate telephone exchanges under the Bell patents, it claimed that this pioneer private exchange was a commercial exchange by reason of connections to outside interests, although calls were exchanged without thought of payment, and thus was infringing the rights of the Connecticut Company. So that exchange was closed. Later on a private branch exchange system was regularly installed for the Ansonia Brass and Copper Company, and now that company is the largest user of telephone equipment and service in Connecticut.

Probably Hartford can be credited with establishing the second mutual telephone exchange system. Three months after Dr. Bell's lecture, an agent for the Bell company called on the principal merchants in Hartford and tried to induce them to utilize the telephone as a business-bringer. On July 19, 1877, the local manager of the Western Union, Mr. G. B. Hubbell, secured the agency of the Bell telephone. On August 9, 1877, the Hartford *Courant* stated: "At the Capital Avenue drug store there is a telephone of simple construction connected with the residence of Dr. Campbell." On August 22 the *Courant* stated that "At the regular meeting of the allopath physicians on Monday evening, experiments were successfully tried with the telephone, and it is proposed to have a system of intercommunication between the doctors established by means of this new invention, so that by reporting to the central office at the Capital Avenue drug store, they can readily exchange views between office and office." In September, 1877, Isaac Smith, owner of the Capital Avenue drug store, had one and possibly two party-lines working in Hartford, and terminating in his store. On October 8, 1877, Smith advertised as fol-

lows: "Prof. Bell's Telephone. I am prepared to build and equip telephone lines at moderate rates. Telegraphic lines, with Morse or other instruments, built of the best material. Please call and examine our telephone lines in operation." In November, 1877, Dr. Crane, a dentist in Hartford, had a party-line on which were six physicians and six druggists, including Smith, and on November 15 Crane advertised: "Messages sent direct from my office to the following places by telephone." On January 24, 1878, the *Courant* announced that "When word came to Hartford of the accident on the Connecticut Western, information was dispatched to the central office from whence run wires to many physicians of this city. In a very short space of time and within a few minutes of each other, nearly a score of doctors and surgeons were at the depot."

As a rule, in the beginning the messages sent over these early telephone lines were not switched through, but received at one telephone by 'Central' and repeated to the subscriber through another telephone. For there was one telephone for each circuit terminating in the central office; if there were six subscriber-lines, then there were six hand telephones hanging on the wall of the central office. But this was not the case at Bridgeport, Ansonia, New Haven or Meriden.

In May, 1877, Mr. Edwin T. Holmes used Bell's hand telephone as an accessory to his central-office burglar-alarm system in Boston, one set of wires serving for both purposes. Five of these alarm wires were cut through a small brass telegraphic pin switchboard, enabling a hand telephone to be connected or plugged-in on any line. Mr. F. E. Kinsman, who was then in the employ of Mr. Holmes, said that (in August, 1877) the service was not given by connecting any two circuits together, but that "it was made by the operator taking the message and turning about and talking through the telephone to the party to whom the message was given." Three months later Mr. Holmes installed hand telephones in many of the wholesale and commission houses to enable these subscribers to the system to notify the Holmes central office to tell the express company to call for packages ready for shipment. Then a central switchboard system was installed and, in March, 1878, there were 256 hand telephones in use. The use of hand telephones only is said to have continued in this system for more than twelve years, although the number of subscribers finally exceeded 500.

THE JEWS: A STUDY OF RACE AND ENVIRONMENT. III.

By Dr. MAURICE FISHBERG
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Mixed Marriages between Persons of Different Christian Denominations

THE assumption that Jews and christians refrain from intermarriage because of an inherent racial antipathy existing between the Aryan and the Semite is disproved by the large number of mixed marriages in western Europe and America. All the facts go far to prove that the only reason why they have not intermarried during the middle ages and even as far as the first half of the nineteenth century was the difference of religious belief. It was both the church and the synagogue which discouraged intermarriage between Jews and christians. Not only has the church prohibited intermarriage with Jews, mohammedans and heathens, but even the adherents of the different christian denominations have been thus enjoined. In the beginning of the nineteenth century intermarriage between catholics and protestants was comparatively rare in Europe and America. With the change of conditions characteristic of our age, a spirit of toleration has become dominant, and mixed marriages are to-day more or less common. In some countries in Europe denominational statistics have been compiled, and these are of considerable interest in this connection. In Hungary, where many religious confessions are represented, the following are the rates of intermarriage: To 100 marriages contracted in 1903 between persons of the same creed there are mixed marriages among unitarians, 167.73; protestants, 49.39; reformed church, 48.52; Greek catholic, 42.79; Greek oriental, 16.88; Jews, 7.21. Here we find a connection between the degree of religious toleration and the proportion of mixed marriages. The unitarian church, which does not prohibit its adherents to marry outside of their faith, shows the highest proportion of mixed marriages of all the other denominations. In fact, there were more mixed than pure marriages. Next come the evangelical and reformed denominations, with nearly 50 mixed to 100 pure marriages. The large proportion of mixed marriages among the Greek catholics is due to intermarriage with adherents of the Roman catholic and Greek oriental churches; comparatively few marry protestants. The Jews and the Roman catholics have the lowest percentage of mixed mar-

riages. It must, however, be mentioned in this connection that in Budapest, the capital of Hungary, where social intercourse between Jews and christians is more common, and the Jews are on a higher economic and intellectual plane than their coreligionists in the province, the rate of intermarriage is nearly double, reaching 17 per cent. in 1904, although it is only ten years since they have been legally permitted to marry with christians. In Germany similar conditions prevail. Of a total number of 468,329 marriages contracted during 1901, only 41,014 were between persons of different faith, *i. e.*, only 9.59 mixed marriages to 100 pure marriages. Among the Jews in that country there were during that year contracted 3,878 pure and 658 mixed marriages, or 16.97 per cent., which shows a larger tendency to intermarry among the Jews than among the general population. Taking the three chief religions in Germany, we find the following proportions of mixed marriages:

Religion and Country	Pure Marriages	Mixed Marriages	No. of Mixed to 100 Pure Marriages
Germany (1901).			
Protestants	277,480	39,115	14.10
Catholics	145,141	39,115	26.95
Jews	3,878	658	16.97
Prussia (1901).			
Protestants	172,570	23,794	13.78
Catholics	87,898	23,794	27.07
Jews	2,570	455	17.71
Bavaria (1901).			
Protestants	12,390	4,588	37.03
Catholics	31,739	4,588	14.45
Jews (1902)	447	42	9.40

It is noteworthy that in Germany catholics are more given to intermarry than the protestants, which is exactly the reverse of conditions in Hungary. To 100 pure catholic marriages, during 1901, there were 26.95 catholics who married protestants, while only 14.1 per cent. of protestants were married to catholics, which is even less than the rate of Jewish mixed marriages in that country, 16.97 per cent. In Prussia also the catholics intermarried more than the protestants and Jews, the proportions being, catholics, 27.07 per cent.; protestants, 13.78 per cent., and Jews, 17.71 per cent. In Bavaria, on the other hand, the reverse is true. There the protestants intermarry to a much larger extent than the catholics. The latter had 37.03 per cent. of mixed marriages, as against only 14.45 per cent. among the former, and 9.4 per cent. among the Jews. It appears from these figures that adherents of the religion of the majority of the inhabitants are less likely to marry outside of their faith than persons following the creeds which are in the minority. Thus in Germany where 62.51 per cent. of the population is protestant, and only 36.06 per cent. catholic, the latter

are more apt to marry protestants; similarly in Prussia (population, 63.29 per cent. protestant and 35.14 per cent. catholic), the catholics have 27.07 per cent. of marriages with protestants as against only 13.78 per cent. of marriages of protestants with catholics. In Berlin, where the population consists of 84.18 per cent. protestants and only 9.98 per cent. catholics, there were in 1904 only 19.93 mixed marriages of protestants to catholics, as against 323.81 per cent. of catholics to protestants, while in Bavaria, having a majority of catholic population, 70.65 per cent., and only 28.32 per cent. of protestants, it is the protestants who have a higher proportion of mixed marriages, 37.03 per cent., as against only 14.45 per cent. among the catholics. The powerful influence of the majority and its tendency to absorb the minority are thus demonstrated. The immediate cause is, of course, to be sought in the fact that there is often some difficulty to find a suitable partner among the minority, and when one is found among the followers of a different creed, all religious scruples are laid aside.[10]

Intermarriage between persons of different creeds is a recent phenomenon, only one hundred years ago it was quite rare. "In no respect has modern civilization acted more beneficently than as promoter of religious toleration," says Westermarck. "In our time difference of faith discourages sympathy to a much less extent than it did in former ages."[11] In Prussia the number of mixed marriages has quadrupled within the last fifty years, while the number of marriages in general has increased only 70 per cent. during that period. In Bavaria the increase has been more pronounced. During the first half of the nineteenth century they constituted less than three per cent. of the total number of marriages, while to-day one in ten marriages is contracted between persons of different faiths.

All these facts and figures emphasize that it was not any racial antipathy between the so-called Semite and Aryan which kept the Jews of former days from marrying with christians. There were practically no mixed marriages among persons of any religion during medieval days. The same prevails to-day in Russia, where mixed marriages are prohibited by law. With the progress of religious toler-

[10] A somewhat similar phenomenon has been noted among the immigrant population in the United States. The Tenth Census made the interesting deduction that in those portions of the country where a single nationality was numerously represented, as, for instance, the Irish in New York City, there was little intermarriage with other nationalities. But where the nationality was not numerously represented, as the Irish in St. Louis, there was a greater tendency among the men to marry native-born women, or women of other nationality. (R. Mayo-Smith, 'Statistics and Sociology,' pp. 111–112.) The same is true of the Jews in the United States: Very few marry christians in New York City, while in the western and southern states intermarriage is common.

[11] Westermarck, The History of Human Marriage, p. 376.

ance characteristic of our age, the number of mixed marriages has increased among persons belonging to all creeds, including the Jews.

Fertility of Mixed Marriages

Besides the religious and sentimental objections which are being made against intermarriage by Jews and christians, many other arguments were brought forward against them. Some have stated that mixed marriages are infertile, that a large proportion remain sterile and that the average number of children born to each marriage is much below the average of pure marriages. It has also been asserted that intermarriage of different races leads to physical, moral and intellectual deterioration. Only few of the virtues of each are inherited, but most of the vices are transmitted to the progeny.

The fertility of mixed marriages has especially been discussed by Joseph Jacobs, who calculated that even if one tenth of all the Jews and Jewesses married outside of their faith, only a little over two per cent. would be left of the original ten per cent. within six generations or 200 years. This idea prevailed, until recently Arthur Ruppin, after a thorough study of Prussian statistics, showed conclusively that there is no real basis for any such assertion. The fertility of mixed marriages in Prussia is not much below that of pure Jewish marriages.

Superficially, statistics would seem to indicate a lower birth rate of Jews married to non-Jews, as can be seen from the following figures, calculated by J. Thon, showing that in Bavaria the fecundity of the various classes of marriages was as follows:

	Pure		
	Christian	Jewish	Mixed
1876–1900	2.64	3.54	1.58
1902	4.40	2.20	1.38
1903	4.31	2.31	2.11

In Prussia and Hungary the same is shown to be the case, while in the city of Berlin the fertility of mixed marriages appears even lower. The percentage of complete sterility is also stated to be higher among mixed than among pure Jewish couples. In New South Wales 13.41 per cent. of all the Jews married to Jewesses were sterile, while among Jews married to christians 30.55 per cent. were sterile. The average number of children was 3.48 among the general population, 4.06 among the Jews and only 2.01 among Jews married to christians.[12]

All these figures are no safe criterion as to the fertility of mixed marriages, because the usual method applied in calculating the material

[12] Results of Census of N. S. W., 1901, Pt. III., 1902, Pt. V., 1903.

is fallacious. The way fertility is calculated is to divide the number of births in a given year by the number of marriages contracted during the same year. Only very few of the births during any one year are due to the marriages during that year, but are from couples married within the preceding twenty-five years. If the number of mixed marriages did not increase, such a division would more or less accurately give us the average fecundity. But as has been shown above, the number of mixed marriages increases regularly in every country considered, so that the births of the year considered represent the fecundity of a smaller number of marriages than have been contracted during this year. A smaller fertility is thus apparently seen among the mixed marriages. I will illustrate this by figures obtained by Ruppin about conditions in Prussia: During 1901 there were 4.2 births to each christian marriage; 2.80 to each Jewish marriage; and only 1.80 to each marriage of a christian to a Jewess and 1.53 to each marriage of a Jew with a christian woman. But recalling that only a few of these births were the results of marriages contracted during 1901, but represent marriages for about twenty-five years, we are led to investigate further. In 1876 only 256 mixed marriages were contracted in Prussia, and during the twenty-five succeeding years they increased annually, reaching 455 in 1901. If we accordingly calculate the birth rate for 1901 on the basis of the average number of marriages during these twenty-five years (1876–1901), the result is entirely different. Ruppin shows that the rates calculated by this method are 5.07 births to each christian marriage, 2.96 to each Jewish marriage, 2.5 to each marriage of a christian with a Jewess and 2.35 to each marriage of a Jew with a christian woman. The difference is thus not much in favor of pure Jewish marriages when compared with mixed. But even this does not give us a clear picture, because in many mixed marriages one of the parties accepts the religion of the other, and the births are then recorded not as the issue of a mixed marriage, but of a pure christian or pure Jewish marriage, as the case may be. Many births resulting from mixed marriages are consequently missing from the official records, thus reducing the average number of births perceptibly. Considering this and, in addition, the fact that most of the mixed marriages occur in large cities, where the birth rates are much lower than in the country, one is bound to agree with Ruppin that the Prussian official statistics do not support the theory that mixed marriages are less fertile than pure mariages.

There is very little to be said about the alleged physical deterioration of the offspring of mixed marriages, because it has not been proved

by any one.[13] Intellectually we do have proofs that the children born to mixed marriages are not below the average of Jewish and christian in Europe. Grant Allen was the first to point out the striking number of distinguished persons of half-Jewish blood as something simply extraordinary.[14] To mention only some of them—Sir John Herschel, the astronomer; Paul Lindau and his brother; G. Ebers, the Egyptologist; Professor Oldenburg, the philologist; Ludovic Halevy, the musician; Paul Heyse; Francis Turner Palgrave, the critic; W. Gifford Palgrave, the traveler; Sir H. Drumond Wolff, Prevost-Paradol; Edwin Booth, the actor; Bret Harte, the novelist, Elie Metchnikoff, the biologist; David Manin; Leon Gambetta; Sir John Millais, the British painter; and many others.

Religion of the Children resulting from Mixed Marriages

The church in many countries often complained that mixed marriages are a net loss to christianity, because the children born to christians married to Jews are more apt to be raised in the tenets of Judaism. The Jews, on the other hand, have always maintained that each marriage of this kind is a distinct loss to Judaism. In fact, it has been pointed out that most of the children are brought up as christians, and that this is a distinct advantage to the Jews, because the race is thus maintained in its desirable purity, unadulterated by the infusion of foreign blood.

From statistical evidence available on the subject, it appears that about 75 per cent. of all the children born to Jews married to christians are baptized immediately at birth, and only 25 per cent. are raised as Jews. This is best seen in Hungary, where the law permitting mixed marriages stipulates that a person intending to marry one of another religion may make provision at the time of making the application for a marriage license about the religion of the children which may be born to them in the future. They may also leave the question open for future consideration, if they so desire. In the latter case it is provided that boys should follow the religion of their father, and girls that of their mother. Of the 3,590 mixed marriages contracted in that country from 1895 to 1903, only 801 have taken advantage of the provision of the law, and decided, at the time they applied for their marriage licenses, about the religious affilia-

[13] If the proportion of stillbirths should be taken as an index of the vitality of the new born, then nothing unfavorable can be found in cases of mixed marriages: In Prussia the percentage of stillbirths was found from 1875 to 1899 as follows: Christians, 3.59 per cent.; Jewish, 3.21 per cent.; and mixed, 3.45 per cent. The rates of the mixed are thus about midway between the pure Jewish and pure christian.

[14] *Mind*, Vol. VIII., pp. 504–5.

tion of their future children. Of these 685, or 85.64 per cent., declared that they desired to bring up their children as christians, and only 116, or 14.36 per cent., decided in favor of the Jewish religion. It is a striking fact that even in cases where the parents registered themselves as freethinkers married to Jews, one half declared their intention to raise their children in the tenets of the christian church. The Jews thus lose in Hungary 85.64 per cent. of the children born of mixed marriages, which is a net gain to the christian church.

In New South Wales, also, it was found that most of the children resulting from mixed marriages are raised as christians. Of the children born to Jews married to christian women, 44 were Jewish and 119 christian; of the children of christians married to Jewesses, 35 were christian and 20 Jewish. The net result is that from the children born to Jewish husbands married to christian wives only 26.99 per cent. were Jewish, and from children born to christian husbands married to Jewish wives 36.36 per cent. are Jewish. It is noteworthy that here the mother has more influence than the father in determining the religion of the children.

In Copenhagen, where the number of mixed marriages is very large, Salomon states that most of the children are raised as christians. The same conditions are stated to prevail in Italy and France.

In Prussia this subject has been studied by the census officials for many years. The result is this: About 75 per cent. of all the children are christian, and only 25 per cent. are Jewish. Thus between 1885 and 1900 the figures are as follows:

	Jewish	Christian
1885	24.78 per cent.	75.22 per cent.
1890	25.48 per cent.	74.52 per cent.
1895	24.47 per cent.	75.53 per cent.
1900	24.21 per cent.	75.79 per cent.

These figures represent the children who lived with their parents in the census years mentioned, and show distinctly that only 25 per cent. of them are Jews, although among the parents 50 per cent. of Jews are represented. It is noteworthy that in Prussia it is the husband's religion which has more influence in determining the religious affiliation of the offspring. During the census of 1900 it was found that in families where the mother was Jewish and the father christian only 20.08 per cent. of the children were Jewish, as against 27.67 per cent. in families where the father was Jewish and the mother christian.

It must also be mentioned that this does not represent the entire loss sustained by the Jews through intermarriage. A person who has one parent of christian origin, even if raised as a Jew, is more likely to marry a christian than a Jew when he becomes of age, because socially he comes in intimate contact with his christian relatives. It is

also not as difficult for him to be baptized, because he considers himself as much of christian as of Jewish origin. It is Ruppin's opinion that hardly 10 per cent. of the children resulting from mixed marriages remain Jews for any considerable length of time. Of these it is doubtful whether any Jews are left after two or three generations.

"The loss sustained by Judaism through mixed marriages," says Ruppin, "is not to be considered a negligible quantity. In 1901, after five years of legalized intermarriage in Hungary, the proportion of children born to mixed parents was 1.23 per cent. of the total number of Jewish births; in Prussia, after twenty-five years of mixed marriages, it was 10.47 per cent., and in Berlin even 15.15 per cent. of all the Jewish births were of mixed origin. Between 1875 and 1902 14,536 children were born in Prussia from mixed marriages." In fact, Ruppin points out that the loss is much greater than through baptism, which is very much in vogue in Prussia. He shows that in Prussia only about 400 Jews are converted annually to christianity, as against 700 children of half-Jewish blood becoming christians. Only about 25 christians are annually converted to Judaism and 75 children of half-Jewish blood are gained by Judaism through intermarriage. In this connection it is of interest to mention the numerous christian missions to the Jews, which are mostly kept up on the moneys contributed by English-speaking people. It is well known that they meet with but little, if any, success. Their expense is enormous. According to Dr. Kohler,[15] the Berlin Society for Promoting Christianity among the Jews spent more than 117,152 Reichsthaler upon the conversion of 461 Jews during the fifty years of its existence, while the London Society paid between the years 1863 and 1894 from £600 to £3,000 for the conversion of a single Jew. It appears from the figures given above that intermarriage brings much better results for the promotion of christianity among the Jews than missions with their 'costly converts,' who only rarely prove to be desirable acquisitions to christianity.

Dissolution of Marriage

There are but few data about the frequency of divorce among the Jews. In eastern Europe and the orient, the rabbinical law of divorce prevails among the Jewish inhabitants, and any trivial cause often suffices before some rabbis to grant a divorce. The husband can even send a divorce to his wife by proxy through a messenger. Divorces, while not uncommon, considering the ease with which they can be obtained, are not very frequent. Family ties are very strong among the eastern European Jews and only rarely is advantage taken of the rabbinical law to dissolve marriage. The western European Jews do

[15] 'Jewish Encyclopedia,' Vol. IV., p. 252.

not depend on the rabbinical divorce law, but follow the civil laws of the countries in which they live. From the few statistical data available it is seen that during the middle of last century divorce was less frequent among the Jews in Bavaria than among the christian population in that country. The same appears to be the case in Berlin, as can be seen from the following figures:

	Average Annual Number of Divorces per 1,000 Population.		
	1885-1886.	1890-1891.	1895-1896.
Protestants	3.57	3.07	4.73
Catholics	2.71	2.81	3.35
Jews	2.67	2.51	3.26

Although the rates are lower among the Jews than among the christians, still there is to be noted a steady increase in the frequency of divorces among them, from 2.67 in 1885 to 3.26 in 1896. Another way to calculate the divorce rate is by taking the number of divorces in any one year per 1,000 married couples found in a city. In Berlin, during the five years, 1895 to 1899, the rates were 3.75 among the Jews and 5.09 among the christians.

In eastern Europe there are more divorces among the Jews in Hungary and Roumania than among the christians. In Hungary, from 1898 to 1901, the divorces among the Jews constituted 9.51 per cent. of the total number of divorces granted in that country, although the Jews only constituted 4.42 per cent. of the total population. In Roumania 5.83 per cent. of the divorces granted during 1897 were granted to Jews, while only 4.55 per cent. of the total population was Jewish.

There are no statistics about divorces among the Jews in the United States, but it can be stated, without fear of meeting serious contradiction, that it is not uncommon. The divorce courts in New York City are quite often asked by Jews to dissolve their marriage. I am inclined to believe that it is in New York as frequent among Jews as among christians. On the east side of the city the immigrant population very often take advantage of the rabbinical law, and easily obtain divorces. But among the native Jews this never happens; they go to the civil courts for the purpose.

From a few scanty statistics about the causes of divorce among Jews, it is evident that there is very little difference between Jews and Christians in this respect. In Berlin, Ruppin mentions that infelicity on the part of the husband is more frequently a cause among Jews than among christians, while infelicity on the part of the wife is more frequent among christians. Wife desertion is also more frequent among Jews than among christians in Berlin.

It has been stated that divorce is more apt to occur among mixed couples than among pure Jewish couples, and some statistics of the city of Berlin support this opinion. During the ten years, 1892 to 1902, to each 1,000 marriages there were divorces as follows: Jews, 3; christians, 3.91; Jews married to christian women, 10.09; christians married to Jewesses, 11.16. Mixed marriages are thus from three to four times more likely to be divorced than pure marriages. Marriages between christians and Jewesses are more often dissolved than marriages between Jews and christian women. Besides the excessive friction incidental between married couples of different faith, even between catholics married to protestants, it must be recalled that mixed marriages are taking place chiefly in large cities, where divorces are more common than in small towns and in the country. Besides, mixed marriages have lately been increasing, as was shown above, and divorces are more frequent among couples recently married than among those who have successfully passed several years of marital life. Statistics of divorce among mixed couples for a small number of years are, therefore, likely to be fallacious, and for a long period of years there are no available data.

With divorces the influence of the social environment is again evident. In the orient and eastern Europe, where the Jews live under strict adherence to their faith and traditions, participating but little in tendencies of modern life, the sacredness of the family ties is strictly guarded, and divorces are uncommon, although easily obtainable.[16] In western Europe and America, where the Jews are completely under the influence of modern city life, divorces are frequent and are growing in frequency.

[16] It will appear from the figures given above about divorces in Hungary and Roumania that they are more frequent among the Jews than among the christians in these countries. But it must be recalled that the christians can not obtain divorces easily according to the civil laws of the country. The Jews, however, are left to themselves in this respect, and they can easily obtain divorces from the rabbis.

PHYSICAL DEGENERACY OR RACE SUICIDE?*

BY SIDNEY WEBB

I.

"IF only the devastating torrent of children could be arrested for a few years," wrote one of the most sympathetic friends of progress, not so very long ago, voicing the opinion of the economists from Malthus to Fawcett, "it would bring untold relief."[1] Not many years have passed, and his aspiration is fulfilled. One of his Majesty's Inspectors of Schools, lately revisiting, after some interval, a public elementary school in the center of London, remarked that, since he was there before, the 'babies' class' had ceased to exist. Between 1896 and 1905 the total population of the County of London is estimated to have increased by 300,000 persons. But the total number of children between three and five years of age who were scheduled by the vigilant school-attendance officers positively fell from 179,426 to 174,359. That this scheduling was fairly exhaustive is shown by the fact that there were almost exactly 5,000 fewer children of that age recorded in the London census of 1901 compared with that of 1891. Nor is this either an isolated or a temporary phenomenon. All over England and Wales the birth rate is falling steadily, in a decline which has already lasted thirty years, and which shows no sign of slackening. In 1876, to every 100,000 of the population there were born 3,630 babies. In 1904, to every 100,000 of the population there were born only 2,790—absolutely the lowest number on record since birth registration began.[2]

What does this continuous fall in the birth rate mean? What will be its results upon our economic and social relationships—what upon the future of the race? What ought we to do, and what can we do, to ward off whatever there is of evil in these results? These are questions which seem to be of greater importance to the community than the ephemeral issues of party politics. Yet those in authority are slow to tackle them. The subject has accordingly been under investigation during the past year by a committee of the Fabian Society; and the present article sets forth some of the results—formulated, however, by the present writer in his own way, for which the society as a whole has no responsibility.

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* From the London *Times*.

[1] 'The Service of Man,' by J. Cotter Morison, preface, p. xx.

[2] Sixty-seventh Annual Report of the Registrar-General, 1906, p. xix.

1. The decline in the birth rate is not merely the result of an alteration in the ages of the population, or in the number or proportion of married women, or in the ages of these.

It is necessary at the outset to remove one possible explanation. What the Registrar-General gives us is the crude birth rate—that is to say, the exact proportion of births during the year to the total population, whether old or young, married or single. But in comparing these birth rates for different years, we have to remember that important changes may take place, even in a single decade, in (*a*) the proportion between children and adults; (*b*) the proportion between married and unmarried; and (*c*) the proportion between married women of the reproductive age and those above that age. These changes—due, it may be, to emigration or immigration, to economic or social developments, or to mere prolongation of the average life—are sufficient, in themselves, to produce a rise or a fall in the crude birth rate, without there having been any increase or decrease in human fertility. To give one striking instance, the crude birth rate of Ireland per 100,000 population fell from 2,384 in 1881, to 2,348 in 1901. But we happen to know that in the course of these twenty years the proportion of married women of reproductive age to the total population so far diminished that the slight fall in the crude birth rate really represented, not a decline, but a positive increase in fertility. If the Ireland of 1901 had contained a population made up, by ages, sexes and marital conditions, in the same proportion as that of 1881, the recorded births in 1901 would have appeared as a birth rate actually higher by 3 per cent. than that of 1881. We have, therefore, first to ask what are the corresponding figures for England and Wales, eliminating all the elements of variations of age, of postponement of marriage, and of positive refusal to marry.[3]

Now, it so happens that this problem has lately been worked out by the statisticians in a way to remove all uncertainty. Dr. Arthur Newsholme and Dr. T. H. C. Stevenson on the one hand, and Mr. G. Udny Yule on the other, have performed the laborious task of 'correcting' the crude birth rates for differences of age, sex and marital conditions, as regards the census years from 1861 to 1901.[4] The results show a

[3] I have restricted myself throughout to legitimate births. The number of illegitimate births in England and Wales is now only 112 per 10,000 of the population, and their omission does not affect the result. Their inclusion would merely have intensified the force of the argument at all points. The corrected illegitimate birth rate fell between 1861 and 1881 by 21 per cent., and between 1881 and 1901 by 41 per cent.—more than twice as fast as the correct legitimate birth rate.

[4] The decline of human fertility in the United Kingdom and other countries as shown by corrected birth rates, by Arthur Newsholme, M.D., medical officer of health, Brighton; and T. H. C. Stevenson, M.D., assistant medical officer to the Education Committee of the London County Council.

definite progressive fall in the proportion of births after allowing for all differences in the way the populations are made up. If the people of England and Wales had continued during those fifty years to be exactly of the same ages, and to be exactly in the same proportion married and single, the birth rate per 100,000 of the population would have changed to the following extent: 1861, 3,236; 1871, 3,312; 1881, 3,273; 1891, 3,125; 1901, 2,729. That is to say, if the fertility of the married women of equivalent ages had remained the same in 1901 as it had been in 1871, there would have been born 3,312 babies per 100,000 population, instead of 2,729, or just upon 21 per cent. more, equal in the whole of England and Wales to something like 200,000 more than actually saw the light. Why were those 200,000 babies not born?

2. The decline in the birth rate is not confined to the towns, nor (so far as England and Wales is concerned, at least) is it appreciably, if any, greater in the towns than it is in the rural districts.

Human fertility may possibly be normally slightly lower in the towns than in the rural districts, and it is sometimes suggested, especially by German authorities, that the fall in the birth rate is to be accounted for by progressive 'urbanization.' But English statistics afford no support to this hypothesis. It is true that the corrected birth rates of the towns of Northampton, Halifax, Burnley and Blackburn fell off between 1881 and 1901 by no less than 32 per cent., and that of London by 16 per cent. But the corrected birth rate of Cornwall fell off by 29 per cent., that of Rutland by 28 per cent., those of Sussex and Devonshire by 26 per cent., and that of Westmorland by 23 per cent. It is no less significant that, whilst the corrected birth rate of all Ireland actually rose during these twenty years by 3 per cent., that of Dublin rose by 9 per cent. If it was the unhealthy environment of our great towns that was causing a reduction in the number of births, we might expect to find Liverpool, Salford, Manchester and Glasgow—cities of extensive overcrowding, fearful slums and high mortality—heading the list. As a matter of fact, the corrected birth rate between 1881 and 1901 fell off proportionately less in these cities than in any other town, and actually less in proportion than in all but six of the counties. A decline in the birth rate, which does not appear at all in Dublin, appears much less in Liverpool and Manchester, Salford and Glasgow than in Brighton, and appears far more in Westmorland, Rutland, Devonshire and Cornwall than in any of those towns, can hardly be due to 'urbanization.'

On the changes in the marriage and birth rates in England and Wales during the past half-century; with an inquiry as to their probable causes, by G. Udny Yule, Newmarch Lecturer in Statistics, University College, London. Both these papers will be found in the *Journal of the Royal Statistical Society*, April, 1906.

3. The decline in the birth rate is exceptionally marked where the inconvenience of having children is specially felt.

There is not much evidence to be adduced under this head, but what there is is of some significance. Where married women habitually go to work in factories, and where their earnings form an important element in the weekly income of the family, the interruption caused by maternity is probably most acutely felt. The enforcement by the factory and workshops acts of 1891 and 1901 of four weeks' absence from employment after child-birth comes as an additional objection. Moreover, in the factory districts the later age at which children can now become productive wage-earners has certainly rendered large families less economically desirable than of yore. It is, therefore, of some significance that the ten towns in all England in which the relative fall in the birth rate between 1881 and 1901 is most startingly great are Northampton, Halifax, Burnley, Blackburn, Derby, Leicester, Bradford, Oldham, Huddersfield and Bolton—all towns in which an exceptionally large proportion of married women are engaged in factory work, in textiles, hosiery or boots. I can adduce no statistics of the decline in the birth rate among the married women teaching in schools; but it is known to be great.

4. The decline in the birth rate appears to be specially marked in places inhabited by the servant-keeping class.

It is significant that Brighton shows a relatively heavy falling off from a birth rate which was already a low one. But a comparison between various districts of London gives us further indications. Let us take, as a convenient index of relative wealth, the percentage of domestic servants to population. The corrected birth rate of Bethnal-green—the district of London in which there are fewest non-Londoners and in which fewest of the inhabitants keep domestic servants—fell off, between 1881 and 1901, by 12 per cent. (or exactly as much as that of the North Riding of Yorkshire). But that of Hampstead—where most domestic servants are kept—fell off by no less than 36 per cent., and attained the distinction of reaching the lowest of all the corrected birth rates that Dr. Newsholme has computed. Second only to Hampstead in this respect come Kensington and Paddington, which have statistically to be taken together, and which, keeping nearly as high a proportion of domestic servants as Hampstead, saw their corrected birth rates, already lower than that of Hampstead, fall off by 19 per cent., and sink to less than two thirds of that of the Bethnal-green of 1881. It would be interesting to extend this comparison, taking all the districts of London in the order of their average poverty, as shown by such indices as the proportion of the inhabitants who live in one- or two-room tenements, by the rateable value per head, and by the percentage keeping domestic servants. But the variations in the registration areas in nearly all these cases prevent accurate comparison of birth

rates between 1881 and 1901. Dr. Newsholme and Dr. Stevenson, on the one hand, and Mr. Udny Yule, on the other, do, indeed, compare the corrected birth rates for 1901 of five separate groups of metropolitan boroughs, arranged in grades of average poverty. This comparison gives us the interesting result that the small group of three 'rich' boroughs have, per 100,000 population (corrected) 2,004 legitimate births; the four groups comprising nineteen intermediate boroughs have almost identical legitimate birth rates of between 2,362 to 2,490 per 100,000; whilst the poorest group of seven boroughs has a legitimate birth rate of no less than 3,078, or 50 per cent. more than that in the 'rich' quarters. From these figures it has been inferred that we are, in London at any rate, multiplying most prolifically from our most inferior stocks. It should, however, be noticed that the group of seven 'poor' boroughs happens to include, not only those containing the greatest numbers of Irish Roman Catholics, but also those in which the great bulk of the Jews are to be found. Practically half the marriages that take place in the registration districts of Whitechapel and Mile-end Old Town are solemnized according to the Jewish rite. It is against all the influences of the Jewish religion, tradition and custom to limit the family, and the birth rate among Jews of all classes and all nationalities is known to be large. We can not, therefore, infer from these statistics, either that the birth rate of the poorest strata of the English race in London is greater than that of the artisan or lower middle class. The remarkable evenness of the corrected birth rate throughout the nineteen 'intermediate' metropolitan boroughs, though they vary from having about 15 up to about 45 per cent. of servant-keeping households, is rather an indication to the contrary. This is in accordance with the fact that the decline in the corrected birth rate appears to be as great in the counties made up preponderatingly of the poorly paid agricultural laborers, as in those districts in which the average level of wages is much higher.[5]

5. The decline in the birth rate appears to be much greater in those sections of the population which give proofs of thrift and foresight than among the population at large.

Here we have to leave the carefully corrected birth rates supplied by Dr. Newsholme, and fall back upon evidence which is statistically less perfect. What would be desirable would be to have precise and 'corrected' birth rates for different years of two sections of the popula-

[5] The failure to take into account the special aggregation of the Jewish and the Irish population in the districts of greatest poverty, and the limitation of the investigation to London, appears to me to diminish the validity of some of Mr. David Heron's implications in the recent publication 'On the Relation of Fertility in Man to Social Status, and on the changes in this relation that have taken place during the last fifty years,' 1906. But these calculations point in the same direction as those cited.

tion, the one comprising those who took thought for the morrow and the other comprising those who did not. Such an exact contrast is, of course, unattainable. But it so happens that we do possess, over a term of nearly forty years, the number of children born in one large sample of the population, selected, it might almost be said, solely by the characteristic of thrift. The Hearts of Oak Friendly Society, the largest centralized benefit society in the kingdom, has now over 272,000 adult male members. This membership belongs to all parts of the United Kingdom, of which it may be said to represent about 3 per cent., or no inconsiderable sample. No one is admitted who is not of good character and in receipt of wages at least 24s. per week, a figure which excludes the agricultural laborer, the unskilled worker in town or country, and even (outside London) the lowest grades of skilled artisans. The society consists, in fact, of the artisan and skilled mechanic class, with some intermixture of the small shopkeeper and others who have risen into the lower middle class. Among its provisions is the 'lying-in benefit,' a payment of 30s. for each confinement of a member's wife. Unfortunately, we do not know either the relative proportions of the members who are married, or the average age of the wives. There is, however, no reason to think that the proportion of married members has appreciably changed, whilst it is believed that the average age of the members as a whole has risen from about 33 to 37.52; and it may possibly be inferred that there is a corresponding increase in the average age of the wives. Judging from the evidence of the Scottish census of 1855,[*] we might in such an event have expected a falling off in the births, due to this assumed difference of age, of at most 15 per cent. Now, what are the facts? From 1866 to 1880 the proportion of lying-in claims to membership rose slowly from 2,176 to 2,472 per 10,000. From 1881 to 1904 it continuously declined, until in the latter year it reached only 1,165 per 10,000 members. The birth rate among the population of a million and a quarter persons, distinguished from the rest, so far as is known, only by one common characteristic, that of thrift, has fallen off between 1881 and 1901 by no less than 46 per cent., or a decline nearly three times as great as that during the same period in England and Wales. Taking the whole period of decline, from 1880 down to the latest year for which I have the statistics, 1904, the falling off is over 52 per cent. A smaller society, the Royal Standard Benefit Society, having 8,225 members and giving a similar benefit, shows similar results. Between 1881 and 1901 the proportion of members claiming the lying-in benefit fell off by more than 56 per cent. If the members of the Hearts of Oak Friendly Society and the Royal Standard Benefit Society had had pro-

[*] See the figures given in 'Fertility, Fecundity and Sterility,' by J. Matthews Duncan, 1871; and those in 'Natality and Fecundity,' by C. J. and J. N. Lewis, 1906, pp. 18, 26 and 33.

portionately as many births in 1904 as the members of 1880 had in that year, there would have been born to them nearly 70,000 babies, instead of 32,000. If the birth rate in these 280,000 families of comparatively prosperous artisans had only fallen in the same degree as that of England and Wales generally, there would have been born to them 58,000 babies instead of 32,000. What was the special influence in these exceptionally thrifty families that prevented the other babies being born?

6. The decline in the birth rate is due to some new cause which was not appreciably operative fifty years ago.

We may, indeed, infer, from the relatively stationary birth-rate, alike of the whole population and of selected classes down to some date between 1861 and 1881, and the steady persistence of the subsequent decline, that the decline is due to some new cause. The same conclusion is reached by the elaborate calculations just published by Mr. Heron.[7]

In 1851, as in 1901, it could have been inferred from a comparison of different districts in the metropolis that 'the more cultured, the more prosperous, healthy, and thrifty classes of the community' were producing fewer children per marriage than the classes of lower social status. But, as regards London in 1851, Mr. Heron is "driven to almost certain conclusion that differences in the mean age of wives were amply sufficient . . . to account for the differential birth rates of districts with divergent social status." The operating cause of a low birth rate was, in fact, at that date, postponement of marriage. We know, however, from Dr. Newsholme's corrected birth rates that no such cause as a greater postponement of mariage, with the corresponding rise in the age of the average wife, has anything to do with the decline in the birth rate now recorded. This decline is due to some cause other than those that were appreciably in operation in 1851.

7. The decline in the birth rate is principally, if not entirely, the result of deliberate volition in the regulation of the marriage state.

The reader can scarcely have read the foregoing statements without coming to the conclusion that the falling-off in the birth rate, which has during the last twenty years deprived England and Wales of some two hundred thousand babies a year, is the result of deliberate intention on the part of the parents. The persistence and universality of the fall in town and country alike; the total absence of any discoverable relation to unhealthy conditions, mental development, the strain of education, town life or physical deterioration of any kind; the remarkable fact that it has been greatest where it is known to be widely desired; the evidence that it accompanies not extreme poverty but social

[7] 'On the Relation of Fertility in Man to Social Status and the changes in this relation that have taken place during the last fifty years.' By David Heron, 1906, p. 20.

well-being, and that it is exceptionally marked where there is foresight and thrift—all this points in one and the same direction.[8]

We may add other evidence. Among the Roman Catholics in the United Kingdom any regulation of the marriage state is strongly forbidden, and has, during recent years, been made the subject of frequent, special animadversion, both privately and from the pulpit. It is significant that Ireland is the only part of the United Kingdom in which the birth rate has not declined; that in Ireland itself it has declined a little in semi-Protestant Belfast, and not at all in Roman Catholic Dublin; and that in the towns of Great Britain the decline is least in Liverpool, Salford, Manchester and Glasgow—towns in which the proportion of Roman Catholics is considerable. Among the principal textile factory towns the decline is least at Preston, which is the one having the largest proportion of Roman Catholics. Among the different metropolitan boroughs—though we can not measure with accuracy the fall in the birth rate—the present rate is highest, and, therefore, in all probability, the fall has been least in those boroughs in which the Irish Roman Catholics (and the Jews who, in this respect, are in the same position) are most numerous. All this is inconsistent with the hypothesis that the decline is due to physical degeneracy, and consistent with that of its being due to deliberate volition. Common report that such deliberate regulation of the marriage state, either with the object of limitation of the family, or (which has the same result) with that of regulating the interval between births, has become widely prevalent during the past quarter of a century—exactly the period of the decline—reaches us from all sides—from doctors and chemists, from the officers of friendly societies and philanthropists working among the poor, and, most significant of all, from those who are engaged in the very extensive business of which this new social practise has given rise. What is needed to complete the demonstration is direct individual evidence.

II.

In the preceding section I have shown, upon statistical evidence that appears to me irresistible in its cumulative force, that the decline in the birth rate which is depriving England and Wales of at least one fifth of every year's normal crop of babies is not accounted for by

[8] It is at any rate consistent with the hypothesis of volitional interference, in view of the fact that illegitimate children are, on an average, certainly less desired than legitimate, that, as already stated, the corrected illegitimate birth rate should have fallen off in England and Wales more than twice as much as the legitimate, and twice as much between 1881 and 1901 as between 1861 and 1881. The figures for Scotland correspond to these. ('Natality and Fecundity,' by C. J. and J. N. Lewis, 1906, p. 54.)

any alteration in the age, sex or marital condition of the population; by any refusal or postponement of marriage; or by any of the effects of 'urbanization' or physical deterioration of sections of the community. The statistical evidence points, in fact, unmistakably to the existence of a volitional regulation of the marriage state that is now ubiquitous throughout England and Wales, among, apparently, a large majority of the population. To verify this inference it seemed necessary to obtain direct individual evidence from a sufficiently large number of persons, taken haphazard from different parts of the country, and from different social grades. This the committee of the Fabian Society set itself to obtain.

The procedure adopted was to obtain a voluntary census from a sufficiently large number of married people who could be relied upon to give frank and truthful answers to a detailed interrogatory. For this information resort was had to between 600 and 700 persons, from whom the committee had grounds of hope that answers would be received. About half of these persons resided in the metropolitan area, the remainder being scattered sparsely over the rest of Great Britain. In social grade, they included a most varied selection of occupations, extending from the skilled artisan to the professional man and the small property owner; omitting, on the one hand, the great army of laborers, and, on the other (with few exceptions), the tiny fraction of the population who have incomes from investments exceeding £1,000 a year. They were, of course, selected without the slightest reference to the subject of the inquiry; so little, indeed, was known about them from this standpoint that more than 20 per cent. of them proved to be unmarried, and thus unable to bear testimony. They were invited to give the information desired without revealing their identity, the form being so arranged as to enable it to be filled up by nothing more easily recognizable than crosses and figures.[9] Altogether 634 forms were sent out. From these there have to be deducted, for one reason or another, 158—viz., 114 bachelors, 30 duplicates (wives of husbands making returns), five which failed to get delivered by post office, two refusals, five returned blank or incomprehensible, and two relating to marriages abroad. Of the 476 remaining, 174 did not reply. Whether these should be added to the number of those who candidly confessed to having taken steps to regulate the births in their families, or to those who had taken no such steps, or in what proportion they should be distributed between the two, the reader must judge for himself. Significant replies were received from 302 persons. But as 14 of the returns included particulars of two marriages, the total number of marriages of which particulars are recorded is 316. In six cases the papers contain references to second marriages of which insufficient particulars

[9] I append the questions asked. (See opposite page.)

are given. These will not, however, materially affect the results. What is recorded here is the result of 316 marriages, and concerns 618 parents—not, of course, an adequate sample of the people of Great Britain, but, being drawn from all parts of the country and from every section of the great 'middle' class, sufficient, perhaps, until more adequate testimony can be obtained, to throw some light on all the previous statistics.

The first division of the marriages is into two classes; marriages with families intentionally limited, and marriages with families not so limited.

In order to avoid clumsy sentences, the term 'limited marriage' will be used to signify a marriage in which the family is intentionally limited, and the term 'unlimited marriage' one in which it has not been so limited. The following table gives all the marriages returned, arranged by the date and classified as limited (L) and unlimited (U),

	Yes.	No.
1. Are you married?..... Those who have been married should return themselves as married. In cases of second marriages each should be dealt with separately. A second paper will be sent if desired.		
2. Is your sex Male?.....		
3. Age last birthday?.....		
4. Date of marriage?..... Further returns from persons married before 1870 are not necessary as the period to be investigated goes back only 30 years.		
5. Age of Husband at Marriage?.....		
6. Age of Wife at Marriage?.....		
7. Particulars of Children born (including still-born children)		

	Date of Birth.	Sex.	Date of Death.*	
	1.	M. F.		
	2.			
	3.			
	4.			
	5.			
	6.			
	7.			
	8.			

* This is only asked for as relevant to the inquiry in cases of death under five years of age.

8. Do you expect to have any more (or any) children?....		
9. In your marriage have any steps been taken to render it childless, or to limit the number of children born?...		
10. If yes, during what years have such steps been taken?..		
11. Has there been any exceptional cause (such as the death or serious illness of husband or wife) tending to the limitation of the number of your children? (If possible, state the cause.)		
12. Observations. Any person willing to add any remarks throwing light on the foregoing return is requested to do so.		

together with (1) the number of childless marriages, (2) the number of children born or intended to be born (less deaths up to the age of five years), and the number of marriages in which more children were anticipated. 'One or two' is printed as one and a half.

Date.				Childless.'			Definite Expected Fertility.			More Children Expected.		
	L.	U.	Total.	L.	U.	Ttl.	L.	U.	Total.	L.	U.	Ttl.
1851	—	1	1	—	—	—	—	6	6	—	—	—
1857	—	1	1	—	—	—	—	9	9	—	—	—
1858	—	1	1	—	—	—	—	6	6	—	—	—
1862	—	2	2	—	—	—	—	11	11	—	—	—
1865	—	1	1	—	—	—	—	7	7	—	—	—
1867	1	1	2	—	—	—	1	5	6	—	—	—
1868	1	1	2	—	—	—	6	4	10	—	—	—
1869	1	1	2	—	—	—	7	1	3	—	—	—
1870	—	1	1	—	—	—	—	1	1	—	—	—
1871	—	1	1	—	—	—	—	6	6	—	—	—
1872	—	2	2	—	—	—	—	12	12	—	—	—
1873	3	3	6	—	—	—	10	12	22	—	—	—
1874	—	1	1	—	1	1	—	—	—	—	—	—
1875	4	1	5	1	1	2	6	—	6	—	—	—
1876	2	—	2	—	—	—	6	—	6	—	—	—
1877	3	—	3	—	—	—	13	—	13	—	—	—
1878	6	2	8	—	—	—	28	6	34	—	—	—
1879	6	—	6	2	—	2	19	—	19	—	—	—
1880	3	—	3	—	—	—	12	—	12	—	—	—
1881	2	—	2	—	—	—	7	—	7	—	—	—
1882	1	2	3	—	—	—	7	5	12	—	—	—
1883	6	2	8	—	1	1	16	3	19	—	—	—
1884	6	2	8	—	—	—	28	3	31	—	—	—
1885	8	2	10	—	—	—	31	8	39	—	—	—
1886	3	1	4	—	—	—	8	7	15	—	—	—
1887	6	2	8	—	1	1	20	2	22	1	1	2
1888	6	2	8	—	1	1	22	4	26	1	—	1
1889	10	3	13	—	1	1	23	4	27	2	1	3
1890	8	—	8	2	—	2	14	—	14	—	—	—
1891	6	1	7	1	1	2	15	—	15	1	—	1
1892	11	—	11	3	—	3	20½	—	20½	—	—	—
1893	11	2	13	2	—	2	23	1	24	3	1	4
1894	7	1	8	2	—	2	10	6	16	1	1	2
1895	16	2	18	5	2	7	22½	—	22½	6	1	7
1896	10	2	12	5	1	6	19	5	24	1	1	2
1897	9	—	9	1	—	1	19	—	19	2	—	2
1898	13	1	14	1	—	1	23	3	26	3	1	4
1899	16	4[10]	20	3	1	4	34	3	37	3	2	5
1900	11	1	12	4	—	4	12½	1	13½	5	—	5
1901	9	2	11	2	1	3	12	2	14	3	1	4
1902	9	6[11]	15	1	2	3	15	6	21	4	3	7
1903	9	6	15	1	2	3	15½	5	20½	3	6	9
1904	9	7	16	2	4	6	12½	3	15½	6	7	13
1905	9	3	9	6	3	9	6	—	6	2	2	4
Undated.	4	—	4	1	—	1	10	—	10	1	—	1
	242	74	316	45	23	68	553½	157	710½	48	28	76

It will be seen of the 316 marriages, 74 are returned as unlimited and 242 as limited. But in order to ascertain the real prevalence of voluntary limitation as affecting population, certain deductions should be made. Marriages prior to 1875 may fairly be taken out, since the

[10] One of these gives no information as to children.

decline of the general birth rate only began after that date. This eliminates six limited and 17 unlimited marriages, leaving 236 limited and 57 unlimited. Again, a usual commencement of limitation appears to be after the birth of at least two children. Marriages contracted in 1903, 1904 and 1905 should, therefore be deducted. This leaves 212 limited and 41 unlimited for the period 1875 to 1902, both years included, and including also four marriages the dates of which were not reported, but which almost certainly fall within the period named. But it must be further noted that no less than 13 of the 41 unlimited marriages were childless, and therefore no occasion for limitation arose, unless the parents had desired a childless marriage. This reduces the number of fertile and unlimited marriages during the period 1875 to 1902 to 28 out of 252, or, if the infertile unlimited marriages are deducted, 239.

If we take the decade 1890–99, which may be regarded as the typical period, we find that out of 120 marriages 107 are limited and 13 unlimited, whilst of these 13 five and possibly six were childless at the date of the return. *In this decade, therefore, only seven or possibly eight unlimited fertile marriages are reported out of a total of 120.*

In order to ascertain the effect of limitation on the size of families let us next take the number of children born and living up to five years of age, of all limited marriages from the earliest recorded (1867) to and including 1903.

NUMBER OF CHILDREN OF LIMITED MARRIAGES

Children in family	0	1	2	3	4	5	6	7	8	9	11
Marriages	39	54	59	29	22	11	6	3	2	1	1
Total Children	0	54	118	87	88	55	36	21	16	9	11

That is, a total of 227 marriages and 495 children. But owing to second marriages, which are not in all cases fully detailed, nine children must be added, together with an uncertain number (say six) for two other fruitful marriages mentioned but not reported. Altogether the parents of these (say) 510 children number 452. This, however, ignores expected children.

Taking all limited marriages we may next ascertain what is the probable total *intended* fertility. We can state the number of each limited family in this form:

Number living added to the number intended where stated; and, secondly, number living *plus* an unspecified addition. Cases where the return says 'two or three' more children expected are classified as 2½, and 'three or four' as 3½. We then get the following results:

TOTAL EXPECTED FERTILITY OF LIMITED MARRIAGES

Intended size of family	3	1	2	2½	3	3½	4	5	6	7	8	9	11
Completed families	33	35	45	4	26	5	21	11	6	2	2	1	0
Families with indeterminate additions	9	17	12	–	6	–	4	1	–	1	0	0	1
	42	52	57	4	32	5	25	12	6	3	2	1	1

If we assume the unspecified addition to average one and a half children we find that the 242 marriages have yielded, or are intended to yield, a total of 619 children and an average of 2.56 children per marriage.

If we take the typical decade 1890–99 we get the following results:

107 LIMITED MARRIAGES, 1890–1899

Children living to the age of five	0	1	2	3	4	5
Marriages	25	23	34	15	6	4
Number of completed families ('no more expected')	22	17	19	9	4	4
Not recorded or doubtful	1	3	5	2	1	–
More expected	2	1	4	3	1	–
Number of children expected where indicated	—	2½	9	1	–	–

This gives 118 living children (excluding deaths of any after five years) and 12 or 13 expected, whilst in 11 cases unspecified additions to the families are anticipated, and 12 cases are doubtful. If one additional child is allowed for each doubtful case and one and a half for each unspecified case, this would give 159 children as the fruit of 107 marriages and of 211 parents (allowing for second marriages, in which cases only three persons are concerned in two marriages). This indicates that the offspring of each limited marriage (judging from the period named) is almost precisely one and a half children per marriage. The average number of children to be expected from each marriage, in England and Wales twenty-five years ago, was at least three times as great!

Information as to the causes which had led to limitation was not specifically asked for. But in many papers a large number of valuable details were supplied. Taking all the limited marriages (242) we find the causes indicated as follows:

CAUSES OF LIMITATION

Economic	38
Sexual ill-health	13
Other ill-health or heredity	19
Disinclination of wife	9
Death of wife	6
Not stated	114
Several causes	43
	242

The death of a parent, of course, is a cause of *limitation* in another sense from that elsewhere employed in this paper.

Analyzing these last again, we find the following causes assigned:

Economic	35	out of	43
Sexual ill-health	11	"	43
Other ill-health or heredity	19	"	43
Disinclination of wife	15	"	43
Death of parent	2	"	43
Other causes	5	"	43

Adding the two together we find that, out of the 128 marriages in which the cause of limitation is stated, the poverty of the parents in relation to their standard of comfort is a factor in 73 cases, sexual ill-health (that is, generally, the disturbing effect of child-bearing) in 24 and the other ill-health of the parents in 38 cases. In 24 cases the disinclination of the wife is a factor, and the death of a parent has in eight cases terminated the marriage. It should be added that in one or two cases of marriages in the earlier years tabulated recent deaths of parents are mentioned which could not have affected the size of the families, and these are not included in the above.

The confidential voluntary census thus taken is, of course, far too small to be, in itself, any proof of a widespread custom. But taken in conjunction with the very extensive statistical evidence already adduced, it seems to me to complete the demonstration. We must, I think, now take it as proved that the principal, if not the sole, cause of the present continuous decline in the birth rate in Great Britain is the deliberate regulation of the marriage state. This practise prevails, it must be inferred, either with the object of family limitation, or merely with that of regulating the intervals between births, among at least one half, and probably among three fourths, of all the married people in Great Britain of reproductive age—not, as is often imagined, only among those above the ranks of labor, but practically among all classes, from the agricultural laborer in sparsely populated districts, and the artisan in the towns, up to the various grades of professional men and even to the wealthy property owners. The result is that after a quarter of a century of this practise, the total number of children born annually in Great Britain is less than four fifths of what it would be if no such interference took place. Nor is the practise confined to this country. The statistics indicate that New South Wales and Victoria have already carried it further than we have, whilst New Zealand is not far behind. Registration in the United States is very imperfect, but it is clear that the American-born inhabitants of New England, and perhaps throughout the whole of the northern states, are rapidly following suit. The same phenomenon is clearly to be traced in the German Empire, especially in Saxony, Hamburg and Berlin, but the German rural districts are as yet unaffected. The Roman Catholic population of Ireland (and of the British cities), as well as those of Canada and Austria, appear to be still almost untouched, but those of Belgium, Bavaria and Italy are beginning to follow in the footsteps of France. The fact that almost every country which has accurate registration is showing a declining birth rate indicates—though, of course, it does not prove—that the practise is becoming ubiquitous.

These clearly proved facts—which we are bound to face whether we like them or not—will appear in different lights to different people. In some quarters it seems to be considered sufficient to dismiss them with

moral indignation, real or simulated. Such a judgment appears to the present writer both irrelevant and futile. It is impossible, as Burke has taught us, to draw an indictment against a whole nation. If a course of conduct is habitually and deliberately pursued by vast multitudes of otherwise well-conducted people, forming probably a majority of the whole educated class of the nation, we must assume that it does not conflict with their actual code of morality. They may be intellectually mistaken, but they are not doing what they feel to be wrong. Assuming, as I think we may, that no injury to physical health is necessarily involved—aware, on the contrary, that the result is to spare the wife from an onerous and even dangerous illness, for which in the vast majority of homes no adequate provision in the way of medical attendance, nursing, privacy, rest and freedom from worry can possibly be made—it is, to say the least of it, difficult on any rationalist morality to formulate any blame of a married couple for the deliberate regulation of their family according to their means and opportunities. Apart from some mystic idea of marriage as a 'sacrament,' or, at any rate, as a divinely instituted relation with peculiar religious obligations for which utilitarian reasons can not be given, it does not seem easy to argue that prudent regulation differs essentially from deliberate celibacy from prudential motives. If, as we have for generations been taught by the economists, it is one of the primary obligations of the individual to maintain himself and his family in accordance with his social position and, if possible, to improve that position, the deliberate restriction of his responsibilities within the means which he has of fulfilling them can hardly be counted otherwise than as for righteousness. And when we pass from obligations of the 'self-regarding' class to the wider conception of duty to the community, the ground for blame is, to the ordinary citizen, no more clear. A generation ago, the economists, and, still more, the 'enlightened public opinion' that caught up their words, would have seen in this progressive limitation of population, whether or not it had their approval, the compensating advantage of an uplifting of the economic conditions of the lowest grade of laborers. At any rate, it would have been said, the poorest will thereby be saved from starvation and famine. To those who still believe in the political economy of Ricardo, Nassau Senior, Cairnes and Fawcett—to those, in fact, who still adhere to an industrial system based exclusively on the pecuniary self-interest of the individual and on unshackled freedom of competition—this reasoning must appear as valid to-day as it did a generation ago.

To the present writer the situation appears in a graver light. More accurate knowledge of economic processes denies to this generation the consolation which the 'Early Victorian' economists found in the limitation of population. No such limitation of numbers prevents the lowest grade of workers, if exposed to unfettered individual competi-

tion, from the horrors of 'sweating' or the terrors of prolonged lack of employment. On the other hand, with factory acts and trade union 'collective bargaining' maintaining a deliberately fixed national *minimum*, the limitation of numbers, however prudent it may be in individual instances, is, from the national standpoint, seen to be economically as unnecessary as it is proved to be futile even for the purposes for which McCulloch and Mill, Cairnes and Fawcett so ardently desired it.

Nor can we look forward, even if we wished to do so, to the vacuum remaining unfilled. It is, as all experience proves, impossible to exclude the alien immigrant. Moreover, there are in Great Britain, as in all other countries, a sufficient number of persons to whom the prudential considerations affecting the others will not appeal, or will appeal less strongly. In Great Britain at this moment, when half, or perhaps two thirds, of all the married people are regulating their families, children are being freely born to the Irish Roman Catholics and the Polish, Russian and German Jews, on the one hand, and to the thriftless and irresponsible—largely the casual laborers and the other denizens of the one-roomed tenements of our great cities—on the other. This particular 25 per cent. of our population, as Professor Karl Pearson keeps warning us, is producing 50 per cent. of our children. This can hardly result in anything but national deterioration; or, as an alternative, in this country gradually falling to the Irish and the Jews. Finally, there are signs that even these races are becoming influenced. The ultimate future of these islands may be to the Chinese.

The conclusion which the present writer draws from the investigation is, however, one of hope, not of despair. It is something to discover the cause of the phenomenon. Moreover, the cause is one that we can counteract. If the decline in the birth rate had been due to physical degeneracy, whether brought about by 'urbanization' or otherwise, we should not have known how to cope with it. But a deliberately volitional interference, due chiefly to economic motives, can at any moment be influenced partly by a mere alteration of the economic conditions, partly by the opportunity for the play of the other motives which will be thereby afforded.

What seems indispensable and urgent is to alter the economic incidence of child-bearing. Under the present social conditions the birth of children in households maintained on less than three pounds a week (and these form four fifths of the nation) is attended by almost penal consequences. The wife is incapacitated for some months from earning money. For a few weeks she is subject to a painful illness, with some risk. The husband has to provide a lump sum for the necessary medical attendance and domestic service. But this is not all. The parents know that for the next fourteen years they will have to dock themselves and their other children of luxuries and even of some of the necessaries of life, just because there will be another mouth to feed.

To four-fifths of all the households in the land each succeeding baby means the probability of there being less food, less clothing, less house room, less recreation and less opportunity for advancement for every member of the family. Similar considerations appeal even more strongly to a majority of the remaining 20 per cent. of the population, who make up the 'middle' and professional classes. Their higher standard of life, with its requirements in the way of culture and refinement, and with the long and expensive education which it demands for their children, makes the advent even of a third or fourth child—to say nothing of the possibility of a family of eight or twelve—a burden far more psychologically depressing than that of the wage-earner. In order that a due number of children may be born, and that they may be born rather of the self-controlled and foreseeing members of each class than of those who are reckless or improvident, we must alter the balance of considerations in favor of the child producing family.

The question is whether we shall be able to turn round with sufficient sharpness and in time. For we have unconsciously based so much of our social policy—so many of our habits, traditions, prejudices and beliefs—on the assumption that the growth of population is always to be reckoned with, and even feared, that a genuine realization of the contrary position will involve great changes. There are thousands of men thinking themselves educated citizens to-day to whose whole system of social and economic beliefs the discovery will be as subversive as was that announced by Copernicus. We may at last understand what the modern economist means when he tells us that the most valuable of the year's crops, as it is the most costly, is not the wheat harvest or the lambing, but the year's quota of adolescent young men and women enlisted in the productive service of the community; and that the due production and best possible care of this particular product is of far greater consequence to the nation than any other of its occupations. Infant mortality, for instance—that terrible and quite needless slaughter within the first twelve months of one seventh of all the babies that are born—is already appealing to us in a new way, though it is no greater than it was a generation ago. We shall suddenly remember, too, that one-third of all the paupers are young children; and we may then realize that it is, to the community, of far more consequence how it shall bring up this quarter of a million children over whom it has complete power than the exact degree of hardness with which it may choose to treat the adults. Instead of turning out the children to tramp with the father or beg with the mother, whenever these choose to take their discharge from the workhouse, which is the invariable practise to-day, we should rather jump at the chance of 'adopting' these unfortunate beings in order to make worthy citizens of them. Half of the young paupers, moreover, are widows' children, bereft of the bread-winner. For them the community will have to arrange to continue in

some form or another the maintenance which the father would have provided had he lived. Above all, we must encourage the thrifty, foreseeing, prudent and self-controlled parents to remove the check which, often unwillingly enough, they at present put on their natural instincts and love of children. We must make it easier for them to undertake family responsibilities. For instance, the arguments against the unlimited provision of medical attendance on the child-bearing mother and her children disappear. We may presently find the leader of the Opposition, if not the Prime Minister, advocating the municipal supply of milk to all infants, and a free meal on demand (as already provided by a far-seeing philanthropist at Paris) to mothers actually nursing their babies. We shall, indeed, have to face the problem of the systematic endowment of motherhood, and place this most indispensable of all professions upon an honorable economic basis. The feeding of all the children at school appears in a new light, and we come, at a stride, appreciably nearer to that not very far distant article in the education code making obligatory in the time-table a new subject—namely, '12 to 1 P.M., table manners (materials provided).' There would be no greater encouragement to parentage in the best members of the middle and upper artisan classes than a great multiplication of maintenance scholarships for secondary, technical and university education, and the multiplication of tax-supported higher schools and colleges at nominal fees, or even free.

Such a revolution in the economic incidence of the burden of child-bearing would leave the way open to the play of the best instincts of mankind. To the vast majority of women, and especially to those of fine type, the rearing of children would be the most attractive occupation, if it offered economic advantages equal to those, say, of school teaching or service in the post office. At present it is ignored as an occupation, unremunerated, and in no way honored by the state. Once the production of healthy, moral and intelligent citizens is revered as a social service and made the subject of deliberate praise and encouragement on the part of the government, it will, we may be sure, attract the best and most patriotic of the citizens. Once set free from the everwhelming economic penalties with which it is at present visited, the rearing of a family may gradually be rendered part of the code of the ordinary citizen's morality. The natural repulsion to interference in marital relations will have free play. The mystic obligations of which the religious-minded feel the force will no longer be confronted by the dead wall of economic necessity. To the present writer it seems that only by some such 'sharp turn' in our way of dealing with these problems can we avoid race deterioration, if not race suicide.

WATERWAY DEFENSES OF THE ATLANTIC COAST

By WILLIAM J. ROE
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IT will be remembered what a spasm of apprehension seized the country when the prospect of war with Spain became imminent. Not only did these fears affect seriously the dwellers in sea-coast cities, but they were shared in to such an extent by those who had been accustomed to plan their summer outings at the sea-shore as to send a very large proportion to the mountains instead. In fact so great was the unreasoning and unreasonable terror that the season of 1898 was quite unremunerative to innkeepers at summer resorts along the coast.

If such was the effect of a declaration of war with Spain, whose sea-power was so notoriously weak, what would it be likely to be in the face of hostilities threatened with a foreign country amply prepared for offensive naval movements? Though the prospect of war with a capable maritime power be ever so remote; though the fashion of modern civilization seems to have been set for arbitration, and congresses of peace have been established with at least the promise of permanency; and though we appear to have entered upon a period of good-will and cordial relations with the most ponderous of nations, still the construction of battle-ships and armored cruisers goes on in every shipyard of every government of the world with increased rather than abated zeal. However much the American public may desire peace, and however determined to exhaust the arts of diplomacy to preserve it, so long as human nature retains any of the virus of the serpent, or the fangs of the wolf, so long will war remain the final appeal of human interest, even though shorn of some of its ferocity as the first resort of inhuman passion. It is, and will continue doubtless for many decades, with nations as with the individual man; that one is always best assured of peace that is best prepared to resist insolence and retaliate forcibly, quickly and effectively upon any form of aggression.

After a long period of lethargy following the civil war the nation has become wisely awakened to the necessity of providing means of defense more in accord with our recent position as a 'world-power.' So have we constructed and are constructing powerful earth-works, replacing the obsolete defenses of stone forts, mounting behind them our guns; so that we planned mines for our harbors, and gathered destructive and far-flying torpedo missiles; and, more important yet, are build-

ing day by day huge monsters of the deep, each succeeding one an improvement in strength and fury upon its predecessor, and all vastly the superiors of those famous ships—the Iowa, the Oregon, the Brooklyn and the rest, that so quickly sent the steel-clad hulls of Spain upon the shoals of the Caribbean Sea.

In the state of high efficiency of modern ordnance, while floating guns are able to throw enormously destructive projectiles to such great distances, it is not beyond the range of reason to declare positively that not a single city upon the Atlantic coast is entirely safe from bombardment by a foreign fleet. The extraordinary contingencies of the Spanish war will not, it may be believed, ever return. The next time (which, with our expanding relations, more and more world-wide continually, may come at any moment) that we are called upon to match strengths with an enemy, we may be quite sure it will be with a foe of different caliber than poor, enervated Spain. Her valor, her deep sense of honor, her devotion, fanatical as that of any follower of Mahomet, all were vain and valueless because of—in one phrase—lack of adequate preparation. The next time the American people are called upon to face an enemy upon the high seas, it will not be, we may be sure, to find his nominal fighting power or the speed of his war ships diminished by so large a percentage; nor, to be frank, that our own ordnance, horse-power of engines, general efficiency, shall again surprise ourselves with performances so much better than was expected or claimed.

Do not let us delude ourselves with the undue confidence that all has been done, or is in process of being done, in the way of adequate preparation. For many years to come, though we construct men-of-war in increasing numbers and with increased power, it will still remain that other nations are also increasing their armaments. That 'next time' it may not be one nation, but a coalition of nations. Besides, in these days of swift changes and sudden inventions, the best efforts of designers of floating fortresses may become obsolete almost overnight.

There are some things, however, that by their very nature can not become obsolete; a single nation may for a time, by reason of greater energy, wealth or genius, so dominate in the game of warfare as to checkmate adversaries right and left. America, with her Ericsson and the Monitor, was for a few years as a queen among pawns. But we could not rely, as no nation can rely, upon such marvels. Little more than forty years have passed and the accepted type of battle-ship is the same all over the world; that has replaced the Miantonoma, as that replaced the Monitor, as that displaced the Congress and the Powhatan. Between the banks of oars of the Carthagenian triremes and the sails of Philip II. and Effingham; between the Seventy-four of 1812 and the 'cheese-box on a raft,' great gulfs of mechanical in-

genuity are fixed. Yet all the while, under all customs of conflict, all political administrations, in every age, certain changeless principles underlie and determine changing methods of action. These things can never become obsolete; the ingenuity by which Rome taught herself to emulate and at last excel in the first Punic war, and that victory comes not so much by the possession of big forces as by having Drakes and Frobishers pitted against a Medina Sidonia.

In every condition of battle, and especially having reference to our own defense on the Atlantic coast from a powerful adversary on the sea, two great principles assert themselves as essential; first is the establishment of defensive relations, by both fortifications and squadrons, and second, the ability to concentrate swiftly and effectively at any threatened point the full measure of naval effectiveness at our command.

A hostile fleet coming upon our coast for purposes of offense would have the advantage of being able to concentrate at any desired point, to select the city that it sought to doom to destruction or spoliation. The timid citizens of New York, who a few years ago had their fears so excited, may take confort in knowing that of all our great sea-board cities, theirs is probably in least danger of bombardment. Gruesome tales were told of the ease with which foreign war ships could float broadside off Coney Island, to send round shot and shell into Broadway and Fifth Avenue. Calm your fears or assuage them; no doubt a hostile fleet can select and concentrate; but of all exposed points it is least likely to choose New York. The reason for this comparative immunity lies in the fact that the Hudson River empties into the ocean at the apex of a reentrant sea-angle, the base of which is found on a line drawn from the end of Long Island at the east to either the Capes of the Delaware or those of the Chesapeake at the south. At Philadelphia and in Hampton Roads are naval stations, and also at Newport and New London. It is reasonable to assume that at all these would be war vessels, which concentrating would be likely to furnish a force to assail an enemy upon the sea in the rear located off Sandy Hook. Formidable or fortunate would an attacking force be to avoid or avert some form of disaster, if not complete destruction. In war, as in the lightning stroke, energy is apt to take the line of least resistance, and it may, I think, be quite confidently asserted that some other city, not possessed of this advantage, would be the one most exposed to attack; Boston and Portland in the north, and on the south Charleston or Savannah.

We shall not enter into any details concerning movements of land forces, nor do more than call attention to the strength of our present sea-coast batteries. I would not lull you to a too great confidence that that strength is sufficient, nor that even torpedoes, fixed or floating, are certainly effective; nor is it necessary to excite further alarm by sug-

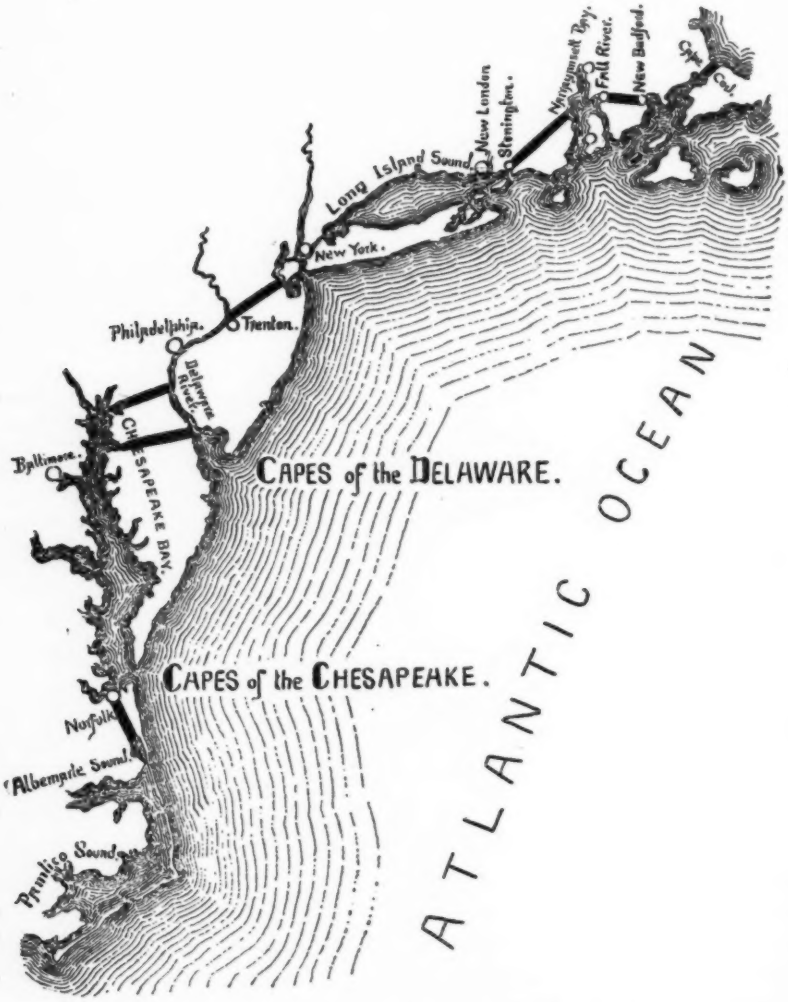
gesting enormous expense involved in the construction of armored forts; these all are foreign to the purpose of this paper, which is simply to point out how easily, and—considering the objects to be achieved—how cheaply, those two principles of defence—mutual defensive relations and concentration of floating forces—can be achieved.

With the map before you note the condition of existing waterways along our seaboard. From the eastern end of Long Island a series of continuous inland waterways begins that, with here and there an interruption, does not end till the coast of Florida is reached. Long Island Sound is now or soon will be amply defended from the sea. The East River, New York Bay, the Kill-von-Kull, the Delaware and Raritan Canal, the Delaware River, the Chesapeake and Delaware Canal, the Chesapeake Bay, the Dismal Swamp Canal, Albermarle Sound, Pamlico Sound and beyond, almost continuous lagoons behind the Sea-Islands of North and South Carolina, Georgia and Florida, complete a chain of channels and artificial canals, awaiting only enlargement, and in some cases adequate or extra fortification to render the entire sea-coast—if not impregnable, at least defensible to an extent to which military men have long been alive; but to which the casual citizens (as well as their representatives in congress assembled) have been not only slumbering, but even in wakeful moments deaf, dumb and blind.

We may go even a little farther in the way of defensive suggestion, by including not only the Atlantic sea front, but also the gulf coast. A canal, surveyed and reported upon long ago by the United States Corps of Engineers, could be constructed at a trivial cost from Jacksonville to and down the Suwanee River. That such a canal has not already been constructed is owing to the fact that in a commercial sense it would not pay. Beyond Fisher's Island at the eastern extremity of Long Island Sound, for purposes of ample inland communication, a ship canal would need to be 'built' from near Watch-Hill to some point on Narragansett Bay, and another from near Fall River to New Bedford. Already a canal (designed for small coasting craft) has been projected and some work done, connecting Buzzard's and Cape Cod Bays. The fact stares us in the face, that with a series of channels requiring only construction or enlargement, connecting navigable and defensible inland bodies of water along the coast, nothing—or comparatively nothing—has been done to effect so great a war benefit. Of course this apathy has been due solely to that narrow commercial instinct—the so-called 'timidity of capital'—which has persisted in refusing even to traffic, to say nothing of national defense, the manifest gain that would come from judicious expenditure. Capital will not (as I have heard it expressed) 'sink money in a ditch.'

Some day, perhaps not in the far future, some of these frugal multi-

millionaires may find occasion to abruptly change their minds. It is 'on the cards,' though they be not yet dealt, not even shuffled. It is not necessary to say that no reference is made especially to Great Britain in quoting a famous phrase: that some day the 'thunders of English cannon may be heard in—St. Angelo?' No, in Boston. When the



splendid Fine Arts building and the Public Library become targets for long range projectiles, and the new 'Old South Church' and Trinity and all the magnificent residences of the 'Back Bay' are crumbling under the 'fire-ball of death,' surely they will be sorry that an expenditure was not demanded, less, far less than the tribute that might be demanded, to save their city from destruction.

Perhaps even before the twentieth century is well out of its youth a hundred millions may not suffice as ransom. That sum, probably much less, spent properly now or within the next five years would go far indeed towards saving their terrors and their pockets.

All this that has been related in perhaps tedious detail has long been under consideration by our department of war. Time and again has laid before the president, the senate and house of representatives the data collected by the engineer corps with painstaking fidelity looking to an end so beneficial. The congress has been asked, urged, implored, in at least one instance where the expenditure required was trifling compared with the defensive result, to construct a deep waterway. Bills, from time to time, have been introduced—five or six in the last fifty years—but nothing has come of any of them of a practical character.

A slight study of the accompanying map showing a portion of our Atlantic seaboard will demonstrate, better perhaps than much argument, the necessity and, inferentially, the effectiveness, of a proposed series of deep water canals, parallel to the coast and connecting one after the other the landlocked and fort-defended rivers, bays and estuaries. From the extreme eastern terminus of the system at Cape Cod Bay, the first of these suggested artificial channels is that which would have its southern end in Buzzard's Bay. Next comes (all being denoted by thick black lines) a similar water communication between New Bedford and Fall River. Still another is proposed between Narragansett Bay and Long Island Sound near Stonington. After this, towards the southeast there is already natural deep-water communication, through the East River and New York harbor. It requires only the widening and deepening of the 'Delaware and Raritan' canal to open a well-defended inland waterway to the Delaware River.

Perhaps at the time we have instanced—not as an alarmist, but as a mere guide-post to possibilities of the future—when a foreign fleet appeared threatening Boston, in New Bedford harbor were a few ironclads. For them to hasten to the threatened point that little strip of sand cut through by a thirty-foot canal would mean perhaps salvation. But with the others cut, how quickly could our fleets gather; one from Newport, another from New London, reinforced—as they speedily would be—by all the naval strength gathered at the New York Navy Yard and at League Island on the Delaware.

But an even more potential presentation of the advantage of a ship-canal of sufficient depth to enable a war ship to pass through it is found in the projected cutting through of the narrow neck that separates the waters of the Delaware from those of the Chesapeake Bay. The suggested waterways between Long Island Sound and Narragansett Bay, and between Fall River and New Bedford have not even

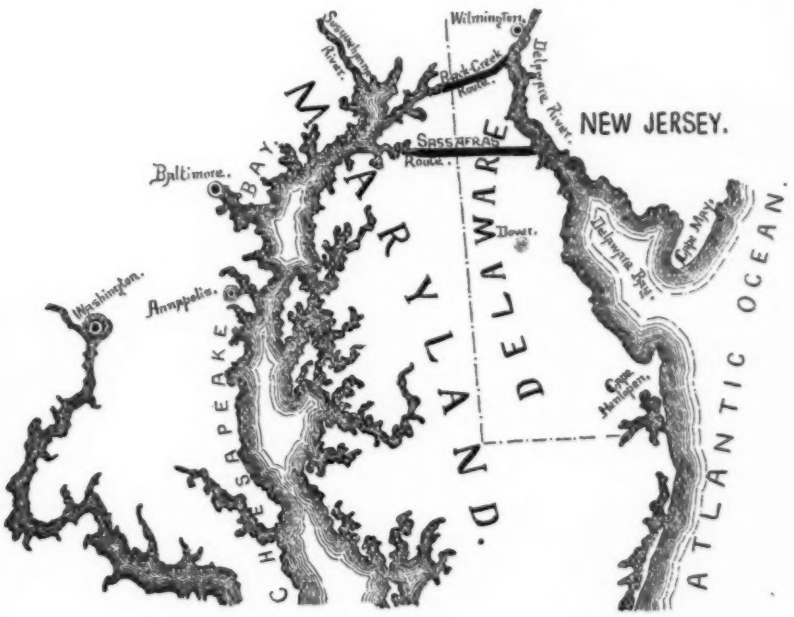
yet been so much as thought of; the Cape Cod cutting, though for over two centuries planned for, and even partially worked, has been regarded solely in the light of a commercial venture. Of all these proposed canals but one has received attention from the United States. This one is the cutting of the divide between the Chesapeake Bay and the Delaware River. In 1894 a commission, authorized by congress and appointed by the president, examined numerous routes already surveyed across the Maryland-Delaware peninsula, with the result that they recommended the route known as the 'Back Creek' or 'Elk River' route, the most northerly of all. Much interest had been taken in a projected canal in Baltimore; but it was wholly in the light of a commercial benefit. The advantage of the route selected—though manifest from a military standpoint—was not perceptible to the practical minds of the traders of the thrifty city. The commission had been required to select that one of six or eight routes (the most southerly being that known as the 'Choptank') which should be most advantageous in ways of commerce as well as those of war.

Baltimore's shippers, willing enough to admit the theoretical benefits of the route selected from a military standpoint, declared with one voice that it possessed none whatever of a commercial character. It would be as well, they said, if not better, to continue to come and go by the old route down the Chesapeake to the capes of Virginia. There was not enough public spirit in congress to incite to action from any purely military considerations; the incentive of private commercial interest being lacking, the project dropped like lead. Ten years passed, and then, at the first session of the fifty-eighth congress a bill was introduced in the house of representatives, and another (at the second session) in the senate, both having for object the purchase by the United States of the 'chartered rights' of the 'Chesapeake and Delaware Canal,' located on the line of the so-called 'Back-Creek,' and the construction of a free ship-canal thereon. Both of these bills failed to get past the committees to which they were referred. Again in 1905 a joint resolution (introduced by Mr. Mudd of Maryland) was referred to the committee on railways and canals. It appeared that this measure was likely to meet the same fate as its predecessors; but interest in the project was aroused in the committee, and intelligent scrutiny; the resolution was modified to the extent that all the so-called 'southern routes' (manifestly of no utility in a military sense) were eliminated, and consideration confined to the two most northerly routes—the 'Back Creek' and the Sassafras. In this shape the joint resolution passed both houses; it was approved by the president, and a commission was appointed to decide which of the two routes was the better, in view of 'probable cost and commercial advantages and military and naval uses of each,' with a view to constructing by the United States of a 'free and open waterway.'

For the first time in the history of these endeavors the question had become a practical one—as to which of two routes was the superior for both commerce and war.

The second of the two maps shows at a glance the situation of the two routes, especially the manifest advantage of the Sassafras, chiefly on account of the greatly decreased distance from the wharves of Baltimore to a point at sea off the capes of the Delaware.

But it is not with the commercial relations of any route that our



interest lies; but rather that by the construction of a ship-canal by one or the other, one link will be securely forged in that chain of waterways by which so much is to be gained in ways of defense of our Atlantic seaboard. Perhaps these defensive advantages could not be better set forth than by quoting in full the 'expert' opinion of General William P. Craighill, former chief of engineers, U. S. A.

NOTES UPON THE MILITARY CONSIDERATIONS CONCERNING A PROPOSED SHIP-CANAL FROM THE DELAWARE TO THE CHESAPEAKE.

It will be doubted by no one that a deep-water communication between the two bays would be of vast importance in the contingency of war with a maritime nation. Such a connection would provide a means of concentrating the floating defenses of the two bays, and besides this would render more secure the communication between the naval stations of Philadelphia and Norfolk and Washington. Vessels defending a port have two offices to perform, the one

being to assist in the direct defense, or to prevent capture or occupation by a hostile force, the other being the prevention or breaking up of blockades. Without a canal a blockade at the Capes of the Delaware would close the port of Philadelphia, or blockade at the Capes of Virginia would close the outward commerce of Baltimore and other ports of the Chesapeake. With the canal built where communication would be secure, neither the ports of Philadelphia nor of Baltimore could be closed unless an effectual blockade were established both at the Delaware and at the Virginia Capes. The disadvantage to the attacking party is obvious, while the defending vessels could concentrate at either outlet, and breaking the blockade at one point would open both ports and render the blockade useless at the other outlet.

When the question of defense is considered in the choice of a route, the elements are rapidity and security of communication. . . . For the purposes of concentration for the defense of the two bays, the Sassafras is superior in regard to rapidity. In this respect there is little or no difference between the Sassafras and the Back Creek.

For security of communication the Sassafras is superior (to the so-called 'lower routes'), as the entire route from Baltimore to Philadelphia can be protected by shore defenses, and the defense can be made or assisted at any point by gun vessels whose light draught would permit them to keep out of water in which they could be rammed by the sea-going warships. The Back-Creek route in this respect is precisely the same as the Sassafras.

The commission, of which General Felix Agnus, of Baltimore, is chairman, has given to the matter of a choice between the two routes all the time and attention that its importance deserves. Public meetings were held during the month of September, in Baltimore, Wilmington and Philadelphia, and great interest manifested, not only on the part of representatives of trade organizations, but by members of congress from the states most directly concerned. This interest was, of course, chiefly commercial, and a keen rivalry was developed between the cities of the Delaware and those of the Chesapeake Bay, the former favoring the line of the present canal, while the Sassafras was advocated by those who foresaw the great benefits that must come to Baltimore from the adoption of this route.

The rivalry, keen as it has been, between the opposed interests of the 'Back-Creek' and the Sassafras routes, has been in all respects honest and good natured; neither side to the controversy having manifested any spirit of jealousy or unwillingness to yield to the findings of the commission.

What that finding may be it is yet too early for conjecture; but in either event, acquiescence will not be withheld, nor all the influences of a united public sentiment, given not 'grudgingly, nor of necessity,' but with generous and cordial assent as to an honest judgment for the public welfare and the nation's good.

THE SIMPLIFICATION OF FRENCH SPELLING

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IN February, 1903, the French government appointed a commission to prepare the simplification of French orthography. It consisted of MM. Bernès, Clairin, Comte, Croiset, Devinat, Gréard, Meyer (members of the Superior Council of Education), M. Havet (of the institute), Professors Brunot and Thomas (of the University of Paris), and MM. Carnaud and Cornet (deputies). M. Paul Meyer was the president of the commission and M. Clairin the secretary. The commission made its report in July, 1904, advocating a series of simplifications of French spelling, in accordance with the principle of omitting useless silent letters—the same principle which is guiding the action of the Simplified Spelling Board here in the United States.

The report of this commission was submitted to the French Academy, which charged M. Emile Faguet with the duty of expressing its opinions. As a result the government appointed a second commission, of which M. Faguet is a member and of which the report was written by Professor Brunot. This report is in type, but it has not yet been distributed. M. Meyer has now reprinted his report, prefacing it with a personal paper of his own in which he discusses the present condition of French orthography, explains the historic reasons for its absurdities and points out how it can most easily be improved. His pamphlet, 'Pour la Simplification de notre Orthographe,' is published in Paris, by Delagrave. His statement of the case is curiously like that which has been made in English by the Simplified Spelling Board.

Ordinarily, spelling is defined as 'the art and science of writing the words of a language correctly, according to established usage.' But that usage becomes established under conditions differing widely, according to the period and the country; and in order to appreciate the value of the orthography of any language it is important in the first place to know the origin of that usage, made permanent in the spelling. Almost everywhere the original idea was that spelling should reflect pronunciation as closely as possible; the phonetic tendency is predominant. But wherever spelling became fixed at an early time, whether by academies or through printers' influence, it ceased gradually to be phonetic in character, because language changed, little by little, in pronunciation as well as in vocabulary and grammar, whereas spelling, once established, paid no attention to these changes. Other causes entered into play which helped gradually to take from the spelling of certain languages the symbol of graphic representation of sounds they originally possessed. One of these causes, and perhaps the most potent, was the pedantry which introduced into the writing of many words so-called etymological letters which were not pronounced. These contradictory tendencies may be seen elsewhere as well as in French. Thus, to cite a single example, English spelling, which was principally phonetic in the sixteenth century, has now become purely conventional, the pronuncia-

tion having undergone since that time considerable change which the written form does not indicate.

M. Paul Meyer then calls attention to the fact that "the great obstacle to the development of a spelling both logical and suited to our language has been the inadequacy of the Latin alphabet, which could not express sounds originating after the Latin period."

In the sixteenth century various expedients were suggested to remedy this poverty of symbols. About 1530, Geoffrey Tory, a printer, introduced the use of the cedilla, already known among the Italians and Spaniards, to indicate the sibilant sound of *c;* but it occurred to no one to employ a similar device to distinguish the two sounds of *g*. Geoffrey Tory also used the *accent aigu* (′), but without giving it exactly the value it has to-day; he made use of it solely to distinguish the *e* pronounced from the *e* mute. The accent grave, which distinguished the open *e* from the closed, was not introduced until much later. In 1562 Ramus succeeded in having the distinctions between *i* and *j*, between *u* and *v*, pass into common use.

He shows clearly that French spelling has suffered from some of the same unfortunate influences which have reduced English spelling to its lamentable condition:

In spite of its lack of uniformity, written French had had until then a phonetic tendency. Unfortunately, there was an antagonistic movement under the influence of humanism, which introduced into the notation of speech certain silent letters to indicate the derivation of words: they wrote *aultre, advocat, doigt, droict, faict, poids, scavoir, soubs, subject*, etc., in order to make the true or supposed etymology of these words visible. This was absurd; there was no need to put an *l* in *autre* to represent that in the Latin *alter*, which was already shown by the *u* (*altre, autre*). These 'superfluities,' so called by the Abbé d'Olivet, editor-in-chief of the third edition of the 'Dictionaire de l'Académie' (1740), in a great many cases, but not in all, have been expunged from the language.

Similar superfluities abound in English still, and they are still defended by arguments like those contained in the preface to the first dictionary of the French Academy (1694). "The Academy adheres to the old spellings accepted among men of letters, because they aid in showing the origin of the words. That is why the academy believes that it ought not to authorize the abridgments which certain individuals, chiefly printers, have made, because these omissions destroy every vestige of the analogy and relation between words that are derived from Latin, or from any other language. Thus the words *corps* and *temps* are given with a *p*, and the words *teste, honneste* with an *s*, to indicate that they come from the Latin *tempus, corpus, testa, honestus.*" As M. Meyer asks, "What value can be given to a spelling founded on such fluctuating principles?" And he quotes Gaston Paris as saying that "the academy, deceived by superficial data, thought it was furthering scientific accuracy by adopting traditional spelling; in reality they followed routine and added to confusion." M. Meyer declares that "what should have been done, had the academy understood its mission, would have been to follow methodically the path taken, instinctively and without purpose, by the writers of the middle ages; a gradual modification of the system of representing sounds was neces-

sary to preserve the connection between spoken and written language. Pronunciation, like vocabulary, alters, insensibly from generation to generation; the written words ought to record each change as it occurs."

Asserting that it is impossible now to inaugurate suddenly a complete series of changes which might readily have been adopted had they been introduced gradually as need arose, M. Meyer tells us that his committee retained existing conventions in so far as they are not in conflict with other conventions no less worthy of consideration, and it denied any wish to establish a phonetic system of spelling. It limited its work to the correction of the most striking irregularities of the present spelling of French. Here again the attitude of this commission of French scholars is seen to be in absolute accord with that taken in America by the Simplified Spelling Board. And the reasons given for action are also almost identical:

1. The esthetic argument: Our spelling is irregular, and gives the language an ugly irregular look.
2. The argument for preservation: It is important to maintain our customary pronunciation upon which the irregularities of our spelling react.
3. The practical argument: These same irregularities make the study of spelling needlessly difficult.

With the contention that certain useless silent letters ought to be retained, because they reveal the derivation, M. Meyer has the impatience of a scholar; and he points out how the existence of these needless letters is dangerous to accuracy of pronunciation:

We write *prompt, promptitude, dompter* (although there was no *p* in the Latin *domitare*), *indomptable*. The Academy says plainly that in the words *indomptable* and *prompt* the *p* is silent. Nevertheless, we hear constantly the pronunciation *dompter, promptitude*, because the school masters who teach French to children, not having the Academy dictionary always at hand, are naturally inclined to pronounce words as they are written. The same cause of error exists in other languages. In English the *g* in *recognize* is pronounced, but formerly it was not even written; the *g* is a pedantic addition which has ended by making its way into the spoken usage. *Fault* and *author* are pronounced as they are written; formerly they were both written and pronounced *faut* and *autor*.

Littré noticed how intimately pronunciation was allied to spelling. They are two forces, he says, which react continually upon each other. When there is no extensive teaching of grammar, and the language is learned orally rather than by the eye, then pronunciation modifies spelling, which follows it closely. When, on the contrary, books play a large part in teaching the mother tongue, spelling influences pronunciation: the tendency is to pronounce all the letters, and traditional pronunciation succumbs in many places to the visible symbols. There are to-day frequent examples of this.

As might be expected, French teachers find the same fault with their illogical spelling that teachers of English find with ours, which is actually far more illogical than theirs and far more irregular. M. Meyer raises their objections with much sympathy:

Although it is to be regretted that children must make such prolonged efforts to learn to write in conformity with obsolete rules a language which they often speak very correctly otherwise, still the misfortune would not be without some compensation if these efforts contributed to the development of their reasoning powers; but they do not. Learning spelling is above all a

matter of memory, especially of the visual memory. Reasoning has nothing to do with it, for there is no reason why *apporter* should be written with two *p's*, while *apercevoir* and *apaiser* should have but one; or why the plural of *chou* and six other words should be formed with an *x*, while all other words terminating in *ou* take *s*, following the ordinary usage. On the other hand, the acquisition of a logical spelling would bring the reasoning powers into play far more than the memory.

But the most forcible passages in M. Meyer's own plea for simplification are in response to the various arguments urged in behalf of the existing orthographic confusion:

The objections advanced by the public to all modification of spelling may be grouped under four heads:

1. Every change in spelling distresses us. We dislike to alter our habits. Books printed in a new spelling are distasteful to us. Besides, we have had enough trouble to learn the current spelling; why should we be obliged to learn another?

2. Words in their very form and independently of their sound, have an esthetic beauty, which would be lost as soon as the appearance was modified.

3. The proposed changes would result, in many cases, in causing all trace of the etymology to disappear. We love the *g* of *doigt*, which reminds us of the Latin *digitus;* the *p* of *temps*, behind which we can see the Latin *tempus;* and *physique* written *fisique* seems barbarous to us. Further, these changes would obscure the derivation of words of the same family.

4. These same changes will lead to lamentable confusion, since we shall no longer be able to distinguish *corps* (a body) written without *p* from *cors* (a corn).

It is apropos of arguments of this kind that a great English linguist, A. J. Ellis, said: "These are very sweeping assertions, and those who have given serious attention to the subject for many years feel astonished that any person of ordinary intelligence and linguistic attainments could commit himself to such statements."

1. Let us take up these feeble arguments one by one. We shall not attempt to dispute the fact that any suddenly introduced innovation shocks us. When the fashion of crinolines came in during the Second Empire it seemed at first utterly ridiculous. But people became accustomed to it; the reason it disappeared was not that people disliked it, but because the fashionable dressmakers had to have new styles. It will be somewhat similar with the alterations in spelling that we propose. Assuredly, they will arouse a more general feeling of instinctive and inexplicable opposition than they would have a hundred years ago, for the number of people who know spelling, or at least who have studied it, is infinitely greater than formerly. The changes introduced by the third edition of the academic dictionary do not seem to have been combatted; those which we are proposing would scarcely have met with objections had they been brought to the attention of the revisers of the sixth edition (1835). But the longer we wait the more difficult it will be both to repair the harm done to the language by the bad spelling in vogue to-day, and to overcome the repugnance to any change. . . . It is inconceivable that, out of respect for opinions which are only prejudices, children and foreigners should be forever condemned to commit to memory complicated and contradictory rules whose only result is to pervert the reasoning faculty. Life is too short to waste a part of it in such absurdities. Besides, the transition will be made in a very simple way which will spare the prejudices of the present generation. . . . When we have to write a book, a memorandum, simply a letter, it is quite certain that we shall not stop at every word to ascertain how it ought to be written, according to the new spelling. In this matter changes can not be imposed by law or by decree, like those regulating public accounts. But although the generation that has reached maturity will continue to follow the usage with which it is familiar from childhood, the younger generation and foreigners who have not unalterable habits will learn the new spelling and spare themselves useless labor. Proof-readers, with the help of a printer's dictionary, will conform to it. . . . Thus the change will take place in the course of a generation, without wounding the deep-rooted sentiments of any one.

2. The second objection may be dealt with briefly. It consists in saying

that the way in which words are written evokes an idea of beauty which these same words would lose if they were written otherwise. The people who have this feeling, which is very difficult to analyze, are stylists, caring more for form than for substance, more for words than for ideas; more for the appearance than for the words themselves. Possibly these same people suppose spelling to be immutable in its nature and fixed by law; the exclusive use of recent editions has left them in ignorance of the fact that many of these very words, admirable in their present form, were written differently by the great writers of the sixteenth and seventeenth centuries. It would be interesting to know if these words as they were written in the manuscripts of these authors would be as beautiful or less beautiful.

3. The objection which maintains that the proposed changes will lessen the etymological value of words, has only an appearance of weight. It is the opinion of those people who wish to exhibit their youthful studies. Such an argument never has been and never will be advanced by a philologist. To begin with, let us bear in mind, among other points, that these etymological traces interest only the small number of those who have studied Greek and Latin, and that it is not quite fair to overload spelling with useless letters, merely for the satisfaction of a few students of etymology. In former times this was all very well when only a small minority knew how to read and write. But to-day spelling is intended for every one.

4. The objection which maintains that different words written alike will be mistaken for one another is so childish that I hesitate to make a serious refutation. Certainly there are those who insist that we shall no longer understand each other if we write *poids* (weight) like *pois* (a vegetable). It is always easy to make puns; but the writers, had they spent a moment's reflection on the subject upon which they give their opinions so freely, would have observed that the French language in its present spelling contains a large number of these homonyms, which are essentially different even though similar in pronunciation and spelling. But these homonyms have never been considered a cause for obscurity. Are there human beings so devoid of sense as to confuse the *poids* (weight) with the *pois* (sweet peas), or *petits-pois* (green peas) when the *d* is omitted from the former? Possibly; but theirs is a case for the expert in mental pathology. . . .

To sum up: it will not be disputed that our orthography is an incoherent mixture of spellings belonging to different epochs, often modeled on conflicting systems. Is it actually possible to reform spelling so as to make it absolutely logical? The committee whose conclusions I have reviewed thinks not, believing that so extended a reform would necessitate a complete revision for which the time is not ripe. But we can at least eliminate from our customary spelling the most hideous anomalies, and, in a word, simplify it. To accomplish this all that is necessary in many cases is to reestablish the old forms, unfortunately altered at the time of the Renaissance, notwithstanding objections from many thoughtful men of that period. In adopting this course we are not revolutionizing the language, as our opponents constantly allege, perpetually confusing language with spelling. We do not even propose radical changes in spelling, and are accepting all its conventions, even when these are not entirely satisfactory. We revert to the true history of the language, lost at many points by ill-chosen innovations. Ours is really a work of preservation. I have shown that not a single objection advanced has any weight. One thing is in our way—habit; that we shall overcome.

In the more formal report to which his own incisive essay is prefixed M. Meyer is able to deal with other aspects of the case. He sees that the prospect of successive reforms in spelling will perhaps alarm those accustomed to consider the manner of writing a language as subject to fixed and immutable rules:

But since it is impossible to hinder the progress of an idiom, and since it is as impossible to establish its pronunciation forever as to exclude it from the vocabulary, it must be admitted that spelling is not a permanent and unchangeable institution—that, on the contrary, from one time to another, it must undergo modifications in order to remain in accord with pronunciation. However, even a superficial acquaintance with the history of our language is suffi-

cient to convince one that there is nothing less unalterable than our spelling. Without going back to early times, when writing was subject to no fixed rule, when each one expressed sounds according to his own pronunciation and following exceedingly vague methods—simply taking as the starting point the first edition of the dictionaire of the French Academy (1694), it is noticeable that each new edition of this dictionary has changed the spelling of numerous words. The third edition (1740), of which the Abbé d'Olivet was the editor-in-chief, altered the spelling of about 5,000 words out of 18,000 included in the dictionary. The fourth (1762), the sixth (1835), the seventh (1878), have continued, within narrower limits, it is true, to modify the spelling. But many of these changes so introduced at different periods, most of which merit approval, have the fault of having been proposed without regard for the whole, and without any certain method. In some words silent letters were eliminated, while in certain others they were allowed to remain. At times, even, by a retrogression, the spelling which had been simplified was again complicated.

The French are a logical race and they have not frightened at a theory as easily as the two peoples who speak English. And, therefore, the report of the French commission reveals the fact that they are looking further into the future than any English-speaking committee would dare to do while retaining the hope of ever achieving any practical result. The French commission ventures to hint at more radical reforms than have entered the minds of our own Simplified Spelling Board. Yet these final suggestions of theirs are as significant of the trend of scientific opinion as they are interesting in themselves:

These are the changes we propose, and which we hope will not be deemed excessive. The committee is not at all insensible to the objections which may be advanced against its work. The chief one is that the proposed alterations are not the result of a system of spelling logically devised, all of whose elements are rigorously coordinated. But it was not the business of the committee to create a new system of spelling; they were simply authorized to remove as far as possible the anomalies which complicate our spelling and render the study of it so difficult for children and foreigners. The committee has had, therefore, to use as a basis the present system of spelling, which represents a bygone condition of the language—and to restrict itself to regulating this system. They themselves admit that they have not even succeeded fully in this modest attempt. In the cases where a rational and uniform notation could not be obtained except by creating new conventional spellings, or at the price of too numerous changes, they refrained, leaving the present spelling intact in spite of its defects. But their self-imposed restraint does not bar subsequent changes. They foresee in the future reforms more general than those they endeavored to prepare by partial changes. Many of the members have even expressed the hope which ought to be recorded here, that some day a new committee composed not only of grammarians but also of phonetic experts may be set to work to develop a system of spelling better adapted than ours to the present state of the language, and sufficiently elastic to follow it through its inevitable changes.

But from now on, important advantages will be secured if the moderate propositions of the committee are accepted. At any rate, the teaching of the language will be greatly facilitated; the number of exceptions that the pupils must learn will be noticeably diminished. Our language will be more easily acquired by foreigners. Finally, by the suppression of inconsistent and obscure forms which make the real pronounciation doubtful, it will be made possible to teach in our schools, that greatly neglected subject, orthoepy. This teaching alone is able to prevent errors in pronunciation which, individual at first, finish by becoming general.

THE VALUE OF SCIENCE

By M. H. POINCARÉ
MEMBER OF THE INSTITUTE OF FRANCE

5. *The Notion of Displacement*

I HAVE shown in 'Science and Hypothesis' the preponderant rôle played by the movements of our body in the genesis of the notion of space. For a being completely immovable there would be neither space nor geometry; in vain would exterior objects be displaced about him, the variations which these displacements would make in his impressions would not be attributed by this being to changes of position, but to simple changes of state; this being would have no means of distinguishing these two sorts of changes, and this distinction, fundamental for us, would have no meaning for him.

The movements that we impress upon our members have as effect the varying of the impressions produced on our senses by external objects; other causes may likewise make them vary; but we are led to distinguish the changes produced by our own motions and we easily discriminate them for two reasons: (1) because they are voluntary; (2) because they are accompanied by muscular sensations.

So we naturally divide the changes that our impressions may undergo into two categories to which perhaps I have given an inappropriate designation: (1) the internal changes, which are voluntary and accompanied by muscular sensations; (2) the external changes, having the opposite characteristics.

We then observe that among the external changes are some which can be corrected, thanks to an internal change which brings everything back to the primitive state; others can not be corrected in this way (it is thus that when an exterior object is displaced, we may then by changing our own position replace ourselves as regards this object in the same relative position as before, so as to reestablish the original aggregate of impressions; if this object was not displaced, but changed its state, that is impossible). Thence comes a new distinction among external changes: those which may be so corrected we call changes of position; and the others, changes of state.

Think, for example, of a sphere with one hemisphere blue and the other red; it first presents to us the blue hemisphere, then it so revolves as to present the red hemisphere. Now think of a spherical vase containing a blue liquid which becomes red in consequence of a chemical reaction. In both cases the sensation of red has replaced that of blue; our senses have experienced the same impressions which have succeeded

each other in the same order, and yet these two changes are regarded by us as very different; the first is a displacement, the second a change of state. Why? Because in the first case it is sufficient for me to go around the sphere to place myself opposite the red hemisphere and reestablish the original red sensation.

Still more; if the two hemispheres, in place of being red and blue, had been yellow and green, how should I have interpreted the revolution of the sphere? Before, the red succeeded the blue, now the green succeeds the yellow; and yet I say that the two spheres have undergone the same revolution, that each has turned about its axis; yet I can not say that the green is to yellow as the red is to blue; how then am I led to decide that the two spheres have undergone the *same* displacement? Evidently because, in one case as in the other, I am able to reestablish the original sensation by going around the sphere, by making the same movements, and I know that I have made the same movements because I have felt the same muscular sensations; to know it, I do not need, therefore, to know geometry in advance and to represent to myself the movements of my body in geometric space.

Another example: An object is displaced before my eye; its image was first formed at the center of the retina; then it is formed at the border; the old sensation was carried to me by a nerve fiber ending at the center of the retina; the new sensation is carried to me by *another* nerve fiber starting from the border of the retina; these two sensations are qualitatively different; otherwise, how could I distinguish them?

Why then am I led to decide that these two sensations, qualitatively different, represent the same image, which has been displaced? It is because I *can follow the object with the eye* and by a displacement of the eye, voluntary and accompanied by muscular sensations, bring back the image to the center of the retina and reestablish the primitive sensation.

I suppose that the image of a red object has gone from the center *A* to the border *B* of the retina, then that the image of a blue object goes in its turn from the center *A* to the border *B* of the retina; I shall decide that these two objects have undergone the *same* displacement. Why? Because in both cases I shall have been able to reestablish the primitive sensation, and that to do it I shall have had to execute the *same* movement of the eye, and I shall know that my eye has executed the same movement because I shall have felt the *same* muscular sensations.

If I could not move my eye, should I have any reason to suppose that the sensation of red at the center of the retina is to the sensation of red at the border of the retina as that of blue at the center is to that of blue at the border? I should only have four sensations qualitatively different, and if I were asked if they are connected by the proportion I have just stated, the question would seem to me ridiculous,

just as if I were asked if there is an analogous proportion between an auditory sensation, a tactile sensation and an olfactory sensation.

Let us now consider the internal changes, that is, those which are produced by the voluntary movements of our body and which are accompanied by muscular changes. They give rise to the two following observations, analogous to those we have just made on the subject of external changes.

1. I may suppose that my body has moved from one point to another but that the same *attitude* is retained; all the parts of the body have therefore retained or resumed the same *relative* situation, although their absolute situation in space may have varied. I may suppose that not only has the position of my body changed, but that its attitude is no longer the same, that, for instance, my arms which before were folded are now stretched out.

I should therefore distinguish the simple changes of position without change of attitude, and the changes of attitude. Both would appear to me under form of muscular sensations. How then am I led to distinguish them? It is that the first may serve to correct an external change, and that the others can not, or at least can only give an imperfect correction.

This fact I proceed to explain as I would explain it to some one who already knew geometry, but it need not thence be concluded that it is necessary already to know geometry to make this distinction; before knowing geometry I ascertain the fact (experimentally, so to speak), without being able to explain it. But merely to make the distinction between the two kinds of change, I do not need to *explain* the fact, it suffices me *to ascertain* it.

However that may be, the explanation is easy. Suppose that an exterior object is displaced; if we wish the different parts of our body to resume with regard to this object their initial relative position, it is necessary that these different parts should have resumed likewise their initial relative position with regard to one another. Only the internal changes which satisfy this latter condition will be capable of correcting the external change produced by the displacement of that object. If, therefore, the relative position of my eye with regard to my finger has changed, I shall still be able to replace the eye in its initial relative situation with regard to the object and reestablish thus the primitive visual sensations, but then the relative position of the finger with regard to the object will have changed and the tactile sensations will not be reestablished.

2. We ascertain likewise that the same external change may be corrected by two internal changes corresponding to different muscular sensations. Here again I can ascertain this without knowing geometry: and I have no need of anything else; but I proceed to give the explanation of the fact employing geometrical language. To go from

the position A to the position B I may take several routes. To the first of these routes will correspond a series S of muscular sensations; to a second route will correspond another series S'' of muscular sensations which generally will be completely different, since other muscles will be used.

How am I led to regard these two series S and S'' as corresponding to the same displacement AB? It is because these two series are capable of correcting the same external change. Apart from that, they have nothing in common.

Let us now consider two external changes: α and β, which shall be, for instance, the rotation of a sphere half blue, half red, and that of a sphere half yellow, half green; these two changes have nothing in common, since the one is for us the passing of blue into red and the other the passing of yellow into green. Consider, on the other hand, two series of internal changes S and S''; like the others, they will have nothing in common. And yet I say that α and β correspond to the same displacement, and that S and S'' correspond also to the same displacement. Why? Simply because S can correct β as well as α and because α can be corrected by S'' as well as by S. And then a question suggests itself: If I have ascertained that S corrects α and β and that S'' corrects α, am I certain that S'' likewise corrects β? Experiment alone can teach us whether this law is verified. If it were not verified, at least approximately, there would be no geometry, there would be no space, because we should have no more interest in classifying the internal and external changes as I have just done, and, for instance, in distinguishing changes of state from changes of position.

It is interesting to see what has been the rôle of experience in all this. It has shown me that a certain law is approximately verified. It has not told me *wherefore* space is, and that it satisfies the condition in question. I knew in fact, before all experience, that space satisfied this condition or that it would not be; nor have I any right to say that experience told me that geometry is possible; I very well see that geometry is possible, since it does not imply contradiction; experience only tells me that geometry is useful.

6. *Visual Space*

Although motor impressions have had, as I have just explained, an altogether preponderant influence in the genesis of the notion of space, which never would have taken birth without them, it will not be without interest to examine also the rôle of visual impressions and to investigate how many dimensions 'visual space' has, and for that purpose to apply to these impressions the definition of § 3.

A first difficulty presents itself: consider a red color sensation affecting a certain point of the retina; and on the other hand a blue color sensation affecting the same point of the retina. It is necessary

that we have some means of recognizing that these two sensations, qualitatively different, have something in common. Now, according to the considerations expounded in the preceding paragraph, we have been able to recognize this only by the movements of the eye and the observations to which they have given rise. If the eye were immovable, or if we were unconscious of its movements, we should not have been able to recognize that these two sensations, of different quality, had something in common; we should not have been able to disengage from them what gives them a geometric character. The visual sensations, without the muscular sensations, would have nothing geometric, so that it may be said there is no pure visual space.

To do away with this difficulty, consider only sensations of the same nature, red sensations for instance, differing one from another only as regards the point of the retina that they affect. It is clear that I have no reason for making such an arbitrary choice among all the possible visual sensations, for the purpose of uniting in the same class all the sensations of the same color, whatever may be the point of the retina affected. I should never have dreamt of it, had I not before learned, by the means we have just seen, to distinguish changes of state from changes of position, that is, if my eye were immovable. Two sensations of the same color affecting two different parts of the retina would have appeared to me as qualitatively distinct, just as two sensations of different color.

In restricting myself to red sensations, I therefore impose upon myself an artificial limitation and I neglect systematically one whole side of the question; but it is only by this artifice that I am able to analyze visual space without mingling any motor sensation.

Imagine a line traced on the retina and dividing in two its surface; and set apart the red sensations affecting a point of this line, or those differing from them too little to be distinguished from them. The aggregate of these sensations will form a sort of cut that I shall call C, and it is clear that this cut suffices to divide the manifold of possible red sensations, and that if I take two red sensations affecting two points situated on one side and the other of the line, I can not pass from one of these sensations to the other in a continuous way without passing at a certain moment through a sensation belonging to the cut.

If, therefore, the cut has n dimensions, the total manifold of my red sensations, or, if you wish, the whole visual space, will have $n + 1$.

Now, I distinguish the red sensations affecting a point of the cut C. The assemblage of these sensations will form a new cut C'. It is clear that this *will divide* the cut C, always giving to the word divide the same meaning.

If, therefore, the cut C' has n dimensions, the cut C will have $n + 1$ and the whole of visual space $n + 2$.

If all the red sensations affecting the same point of the retina were

regarded as identical, the cut C' reducing to a single element would have 0 dimension, and visual space would have 2.

And yet most often it is said that the eye gives us the sense of a third dimension, and enables us in a certain measure to recognize the distance of objects. When we seek to analyze this feeling, we ascertain that it reduces either to the consciousness of the convergence of the eyes, or to that of the effort of accommodation which the ciliary muscle makes to focus the image.

Two red sensations affecting the same point of the retina will therefore be regarded as identical only if they are accompanied by the same sensation of convergence and also by the same sensation of effort of accommodation or at least by sensations of convergence and accommodation so slightly different as to be indistinguishable.

On this account the cut C' is itself a continuum and the cut C has more than one dimension.

But it happens precisely that experience teaches us that when two visual sensations are accompanied by the same sensation of convergence, they are likewise accompanied by the same sensation of accommodation. If then we form a new cut C'' with all those of the sensations of the cut C', which are accompanied by a certain sensation of convergence, in accordance with the preceding law they will all be indistinguishable and may be regarded as identical. Therefore C'' will not be a continuum and will have 0 dimension; and as C'' divides C' it will thence result that C' has one, C two and *the whole visual space three dimensions.*

But would it be the same if experience had taught us the contrary and if a certain sensation of convergence were not always accompanied by the same sensation of accommodation? In this case two sensations affecting the same point of the retina and accompanied by the same sense of convergence, two sensations which consequently would both appertain to the cut C'' could nevertheless be distinguished since they would be accompanied by two different sensations of accommodation. Therefore C'' would be in its turn a continuum and would have one dimension (at least); then C' would have two, C three and *the whole visual space would have four dimensions.*

Will it then be said that it is experience which teaches us that space has three dimensions, since it is in setting out from an experimental law that we have come to attribute three to it? But we have therein performed, so to speak, only an experiment in physiology; and as also it would suffice to fit over the eyes glasses of suitable construction to put an end to the accord between the feelings of convergence and of accommodation, are we to say that putting on spectacles is enough to make space have four dimensions and that the optician who constructed them has given one more dimension to space? Evidently not; all we

can say is that experience has taught us that it is convenient to attribute three dimensions to space.

But visual space is only one part of space, and in even the notion of this space there is something artificial, as I have explained at the beginning. The real space is motor space and this it is that we shall examine in the following chapter.

Chapter IV. Space and its Three Dimensions

§ 1. *The Group of Displacements*

Let us sum up briefly the results obtained. We proposed to investigate what was meant in saying that space has three dimensions and we have asked first what is a physical continuum and when it may be said to have n dimensions. If we consider different systems of impressions and compare them with one another, we often recognize that two of these systems of impressions are indistinguishable (which is ordinarily expressed in saying that they are too close to one another, and that our senses are too crude, for us to distinguish them) and we ascertain besides that two of these systems can sometimes be discriminated from one another though indistinguishable from a third system. In that case we say the manifold of these systems of impressions forms a physical continuum C. And each of these systems is called an *element* of the continuum C.

How many dimensions has this continuum? Take first two elements A and B of C, and suppose there exists a series Σ of elements, all belonging to the continuum C, of such a sort that A and B are the two extreme terms of this series and that each term of the series is indistinguishable from the preceding. If such a series Σ can be found, we say that A and B are joined to one another; and if any two elements of C are joined to one another, we say that C is all of one piece.

Now take on the continuum C a certain number of elements in a way altogether arbitrary. The aggregate of these elements will be called a *cut*. Among the various series Σ which join A to B, we shall distinguish those of which an element is indistinguishable from one of the elements of the cut (we shall say that these are they which *cut* the cut) and those of which *all* the elements are distinguishable from all those of the cut. If *all* the series Σ which join A to B cut the cut, we shall say that A and B are *separated* by the cut, and that the cut *divides* C. If we can not find on C two elements which are separated by the cut, we shall say that the cut *does not divide* C.

These definitions laid down, if the continuum C can be divided by cuts which do not themselves form a continuum, this continuum C has only one dimension; in the contrary case it has several. If a cut forming a continuum of 1 dimension suffices to divide C, C will have 2 dimensions; if a cut forming a continuum of 2 dimensions suffices, C will have 3 dimensions, etc. Thanks to these definitions, we can always

recognize how many dimensions any physical continuum has. It only remains to find a physical continuum which is, so to speak, equivalent to space, of such a sort that to every point of space corresponds an element of this continuum, and that to points of space very near one another correspond indistinguishable elements. Space will have then as many dimensions as this continuum.

The intermediation of this physical continuum, capable of representation, is indispensable; because we can not represent space to ourselves, and that for a multitude of reasons. Space is a mathematical continuum, it is infinite, and we can represent to ourselves only physical continua and finite objects. The different elements of space, which we call points, are all alike, and, to apply our definition, it is necessary that we know how to distinguish the elements from one another, at least if they are not too close. Finally absolute space is nonsense, and it is necessary for us to begin by referring space to a system of axes invariably bound to our body (which we must always suppose put back in the initial attitude).

Then I have sought to form with our visual sensations a physical continuum equivalent to space; that certainly is easy and this example is particularly appropriate for the discussion of the number of dimensions; this discussion has enabled us to see in what measure it is allowable to say that 'visual space' has three dimensions. Only this solution is incomplete and artificial. I have explained why, and it is not on visual space, but on motor space that it is necessary to bring our efforts to bear. I have then recalled what is the origin of the distinction we make between changes of position and changes of state. Among the changes which occur in our impressions, we distinguish, first the *internal* changes, voluntary and accompanied by muscular sensations, and the *external* changes, having opposite characteristics. We ascertain that it may happen that an external change may be *corrected* by an internal change which reestablishes the primitive sensations. The external changes capable of being corrected by an internal change are called *changes of position,* those not capable of it are called *changes of state.* The internal changes capable of correcting an external change are called *displacements of the whole body;* the others are called *changes of attitude.*

Now let α and β be two external changes, α' and β' two internal changes. Suppose that α may be corrected either by α' or by β', and that α' can correct either α or β; experience tells us then that β' can likewise correct β. In this case we say that α and β correspond to the *same* displacement and also that α' and β' correspond to the *same* displacement. That postulated, we can imagine a physical continuum which we shall call *the continuum or group of displacements* and which we shall define in the following manner. The elements of this continuum shall be the internal changes capable of correcting an external

change. Two of these internal changes α' and β' shall be regarded as indistinguishable: (1) if they are so naturally, that is, if they are too close to one another; (2) if α' is capable of correcting the same external change as a third internal change naturally indistinguishable from β'. In this second case, they will be, so to speak, indistinguishable by convention, I mean by agreeing to disregard circumstances which might distinguish them.

Our continuum is now entirely defined, since we know its elements and have fixed under what conditions they may be regarded as indistinguishable. We thus have all that is necessary to apply our definition and determine how many dimensions this continuum has. We shall recognize that it has *six*. The continuum of displacements is, therefore, not equivalent to space, since the number of dimensions is not the same; it is only related to space. Now how do we know that this continuum of displacements has six dimensions? We know it *by experience*.

It would be easy to describe the experiments by which we could arrive at this result. It would be seen that in this continuum cuts can be made which divide it and which are continua; that these cuts themselves can be divided by other cuts of the second order which yet are continua, and that this would stop only after cuts of the sixth order which would no longer be continua. From our definitions that would mean that the group of displacements has six dimensions.

That would be easy, I have said, but that would be rather long; and would it not be a little superficial? This group of displacements, we have seen, is related to space, and space could be deduced from it, but it is not equivalent to space, since it has not the same number of dimensions; and when we shall have shown how the notion of this continuum can be formed and how that of space may be deduced from it, it might always be asked why space of three dimensions is much more familiar to us than this continuum of six dimensions, and consequently doubted whether it was by this detour that the notion of space was formed in the human mind.

§ 2. *Identity of Two Points*

What is a point? How do we know whether two points of space are identical or different? Or, in other words, when I say: The object A occupied at the instant α the point which the object B occupies at the instant β, what does that mean?

Such is the problem we set ourselves in the preceding chapter, § 4. As I have explained it, it is not a question of comparing the positions of the objects A and B in absolute space; the question then would manifestly have no meaning. It is a question of comparing the positions of these two objects with regard to axes invariably bound to my body, supposing always this body replaced in the same attitude.

I suppose that between the instants α and β I have moved neither my body nor my eye, as I know from my muscular sense. Nor have I moved either my head, my arm or my hand. I ascertain that at the instant α impressions that I attributed to the object A were transmitted to me, some by one of the fibers of my optic nerve, the others by one of the sensitive tactile nerves of my finger; I ascertain that at the instant β other impressions which I attribute to the object B are transmitted to me, some by this same fiber of the optic nerve, the others by this same tactile nerve.

Here I must pause for an explanation; how am I told that this impression which I attribute to A, and that which I attribute to B, impressions which are qualitatively different, are transmitted to me by the same nerve? Must we suppose, to take for example the visual sensations, that A produces two simultaneous sensations, a sensation purely luminous a and a colored sensation a', that B produces in the same way simultaneously a luminous sensation b and a colored sensation b', that if these different sensations are transmitted to me by the same retinal fiber, a is identical with b, but that in general the colored sensations a' and b' produced by different bodies are different? In that case it would be the identity of the sensation a which accompanies a' with the sensation b which accompanies b', which would tell that all these sensations are transmitted to me by the same fiber.

However it may be with this hypothesis and although I am led to prefer to it others considerably more complicated, it is certain that we are told in some way that there is something in common between these sensations $a + a'$ and $b + b'$, without which we should have no means of recognizing that the object B has taken the place of the object A.

Therefore I do not further insist and I recall the hypothesis I have just made: I suppose that I have ascertained that the impressions which I attribute to B are transmitted to me at the instant β by the same fibers, optic as well as tactile, which, at the instant α, had transmitted to me the impressions that I attributed to A. If it is so, we shall not hesitate to declare that the point occupied by B at the instant β is identical with the point occupied by A at the instant α.

I have just enunciated two conditions for these points being identical; one is relative to sight, the other to touch. Let us consider them separately. The first is necessary, but is not sufficient. The second is at once necessary and sufficient. A person knowing geometry could easily explain this in the following manner: Let 0 be the point of the retina where is formed at the instant α the image of the body A; let M be the point of space occupied at the instant α by this body A; let M' be the point of space occupied at the instant β by the body B. For this body B to form its image in 0, it is not necessary that the points M and M' coincide; since vision acts at a distance, it suffices for the

three points 0 M M' to be in a straight line. This condition that the two objects form their image on 0 is therefore necessary, but not sufficient for the points M and M' to coincide. Let now P be the point occupied by my finger and where it remains, since it does not budge. As touch does not act at a distance, if the body A touches my finger at the instant α, it is because M and P coincide; if B touches my finger at the instant β, it is because M' and P coincide. Therefore M and M' coincide. Thus this condition that if A touches my finger at the instant α, B touches it at the instant β, is at once necessary and sufficient for M and M' to coincide.

But we who, as yet, do not know geometry can not reason thus; all that we can do is to ascertain experimentally that the first condition relative to sight may be fulfilled without the second, which is relative to touch, but that the second can not be fulfilled without the first.

Suppose experience had taught us the contrary, as might well be; this hypothesis contains nothing absurd. Suppose, therefore, that we had ascertained experimentally that the condition relative to touch may be fulfilled without that of sight being fulfilled, and that, on the contrary, that of sight can not be fulfilled without that of touch being also. It is clear that if this were so we should conclude that it is touch which may be exercised at a distance, and that sight does not operate at a distance.

But this is not all; up to this time I have supposed that to determine the place of an object, I have made use only of my eye and a single finger; but I could just as well have employed other means, for example, all my other fingers.

I suppose that my first finger receives at the instant α a tactile impression which I attribute to the object A. I make a series of movements, corresponding to a series S of muscular sensations. After these movements, at the instant α, my *second* finger receives a tactile impression that I attribute likewise to A. Afterwards, at the instant β, without my having budged, as my muscular sense tells me, this same second finger transmits to me anew a tactile impression which I attribute this time to the object B; I then make a series of movements, corresponding to a series S' of muscular sensations. I know that this series S' is the inverse of the series S and corresponds to contrary movements. I know this because many previous experiences have shown me that if I made successively the two series of movements corresponding to S and to S', the primitive impressions would be reestablished, in other words, that the two series mutually compensate. That settled, should I expect that at the instant β', when the second series of movements is ended, my *first finger* would feel a tactile impression attributable to the object B?

To answer this question, those already knowing geometry would reason as follows: There are chances that the object A has not budged,

between the instants α and α', nor the object B between the instants β and β'; assume this. At the instant α, the object A occupied a certain point M of space. Now at this instant it touched my first finger, and *as touch does not operate at a distance,* my first finger was likewise at the point M. I afterward made the series S of movements and at the end of this series, at the instant α', I ascertained that the object A touched my second finger. I thence conclude that this second finger was then at M, that is, that the movements S had the result of bringing the second finger to the place of the first. At the instant β the object B has come in contact with my second finger: as I have not budged, this second finger has remained at M; therefore the object B has come to M; by hypothesis it does not budge up to the instant β'. But between the instants β and β' I have made the movements S'; as these movements are the inverse of the movements S, they must have for effect bringing the first finger in the place of the second. At the instant β' this first finger will, therefore, be at M; and as the object B, is likewise at M, this object B will touch my first finger. To the question put, the answer should, therefore, be yes.

We who do not yet know geometry can not reason thus; but we ascertain that this anticipation is ordinarily realized; and we can always explain the exceptions by saying that the object A has moved between the instants α and α', or the object B between the instants β and β'.

But could not experience have given a contrary result? Would this contrary result have been absurd in itself? Evidently not. What should we have done then if experience had given this contrary result? Would all geometry thus have become impossible? Not the least in the world. We should have contented ourselves with concluding that *touch can operate at a distance.*

When I say, touch does not operate at a distance, but sight operates at a distance, this assertion has only one meaning, which is as follows: To recognize whether B occupies at the instant β the point occupied by A at the instant α, I can use a multitude of different criteria. In one my eye intervenes, in another my first finger, in another my second finger, etc. Well, it is sufficient for the criterion relative to one of my fingers to be satisfied in order that all the others should be satisfied, but it is not sufficient that the criterion relative to the eye should be. This is the sense of my assertion, I content myself with affirming an experimental fact which is ordinarily verified.

At the end of the preceding chapter we analyzed visual space; we saw that to engender this space it is necessary to bring in the retinal sensations, the sensation of convergence and the sensation of accommodation; that if these last two were not always in accord, visual space would have four dimensions in place of three; we also saw that if we brought in only the retinal sensations, we should obtain 'simple visual space,' of only two dimensions. On the other hand, consider tactile

space, limiting ourselves to the sensations of a single finger, that is in sum the assemblage of positions this finger can occupy. This tactile space that we shall analyze in the following section and which consequently I ask permission not to consider further for the moment, this tactile space, I say, has three dimensions. Why has space properly so called as many dimensions as tactile space and more than simple visual space? It is because touch does not operate at a distance, while vision does operate at a distance. These two assertions have the same meaning and we have just seen what this is.

Now I return to a point over which I passed rapidly in order not to interrupt the discussion. How do we know that the impressions made on our retina by A at the instant α and B at the instant β are transmitted by the same retinal fiber, although these impressions are qualitatively different? I have suggested a simple hypothesis, while adding that other hypotheses, decidedly more complex, would seem to me more probably true. Here then are these hypotheses, of which I have already said a word. How do we know that the impressions produced by the red object A at the instant α, and by the blue object B at the instant β, if these two objects have been imaged on the same point of the retina, have something in common? The simple hypothesis above made may be rejected and we may suppose that these two impressions, qualitatively different, are transmitted by two different though contiguous nervous fibers. What means have I then of knowing that these fibers are contiguous? It is probable that we should have none, if the eye were immovable. It is the movements of the eye which have told us that there is the same relation between the sensation of blue at the point A and the sensation of blue at the point B of the retina as between the sensation of red at the point A and the sensation of red at the point B. They have shown us, in fact, that the same movements, corresponding to the same muscular sensations, carry us from the first to the second, or from the third to the fourth. I do not emphasize these considerations, which belong, as one sees, to the question of local signs raised by Lotze.

VESUVIUS DURING THE EARLY MIDDLE AGES

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IT is certain that Vesuvius, prior to the Plinian eruption of 79 A. D., by far the most tragic, and one of the three most violent in Italian history, was regarded as an entirely extinct volcano. The details of this eruption, the sequence of its phenomena, and its peculiarly destructive effects, are familiar to us from contemporary sources, and from the memorials written in large characters by the mountain itself over the ruined cities at its base. From the date of this catastrophe onward for over fifteen hundred years, when the period of modern investigation begins, our knowledge of Vesuvian history depends upon more or less casual mention, and upon brief notices of eruptions in monastic chronicles.

Owing to the scantiness of our information, little attention has been paid by students to the long interval separating the two most violent paroxysms known to have shaken the mountain. Yet, inadequate as the records are, their importance is of the first order. They register for us the dates of major disturbances, at least, extending over a period of sixteen centuries, and afford some means for estimating the intensity of volcanic action in the Naples district for a still longer period. Moreover, they furnish data for reconstructing the probable form of the mountain in antiquity, and for detecting the amount of change it has undergone since Plinian times. Nor should it be forgotten that the early topographic descriptions that have come down to us offer interesting points of comparison with the present condition of the stately guardian of the Bay of Naples.

Thus it appears that the original sources of information, which are all that need concern us in the present article, acquaint us not only with the actual history of Vesuvius since the first century of our era, but, taken in connection with other facts, throw a fresh coloring upon the accounts of the 'burnt mountain' that have survived from classic times. Two of the points just enumerated will repay further inquiry: first, the chronology of eruptions during the early middle ages; and secondly, the probable form of Somma-Vesuvius in antiquity. One reason why a review of the chronology seems desirable is because the dates of medieval eruptions are often confused, and the authorities for them incorrectly given, or more frequently omitted. It will be profitable, therefore, to take a brief survey of the original sources, but without

entering into the voluminous literature of the Plinian disaster, which belongs in a separate category. Note, however, that some of the titles included under it are luminous for an understanding of early Somma-Vesuvius history, and the same may be said of Cassius Dio's account of the second eruption (203 or 204), which also falls outside the period of our inquiry.

The whole history of Italy under the Goths is contained in Cassiodorus and Procopius, although the dry compendium of Marcellinus Comes is not without value for the chronology of certain facts. These three writers are our only informants[1] of the eruption that fell at the beginning of the dark ages, in 472, and a word or two concerning them may not be inapt. Magnus Aurelius Cassiodorus, a great Roman noble of wealth, learning and astute statesmanship, born in 480 and reputed to have lived nearly a century, occupies a position throughout the reign of Theodoric scarcely less prominent than that of the king himself, whose chief counsellor he was. His writings, especially the 'Variæ,' or collection of state papers, are of incalculable value for Italian history under Gothic rule, and contain a wealth of curious detail concerning political, social and moral conditions, and general life of the period. In these official papers, the secretary frequently intersperses comments, from an obviously personal point of view, upon any subject that interests him, often displaying remarkable erudition. One of his marked tendencies is a passion for natural history, which he touches upon with naïve ardor, yet displaying withal acute observation. Many a random note occurs relating to birds, beasts or fishes, as witness for example his excursus on the elephant, faintly suggestive of Ctesias, or his description of the 'exormiston,' identified by Dr. Theodore Gill with a Leptocephalus. Little wonder is it, therefore, that we find in these 'Variæ' (iv., 50) an interesting digression on Vesuvius, apropos of an eruption commonly assigned to the year 512, but which, according to Mommsen,[2] must have taken place from one to five years earlier. The date of this event is accordingly best written 507/511. Allusion is also made to the far more severe eruption of 472, remarkable for its heavy discharge of ashes, carried to an enormous distance. For years afterward at Constantinople a solemn fast was held on the sixth of November in memory of that day when the heavens were darkened, and the greater part of Asia Minor was rocking with frightful earthquake shocks. In another letter (iii., 47) he refers to an eruption of one of

[1] Pious imagination of later days has added much fanciful embroidery to the accounts of this and other early eruptions, coupled with the miraculous intervention of Naples' patron saint. The curious will find entertaining reading in the various lives of St. Januarius, as, for instance, that by Girolamo, 1733.

[2] 'Monumenta Germaniæ Historica, Auct. Antiq.,' Vol. XII. (1894), p. 137. We shall refer hereinafter to the folio volumes of the 'Scriptores' series under the abbreviation *M. G. H., SS.*

the Lipari Islands, the date of which is said to have coincided with Hannibal's death, 183 B. C.

So far Cassiodorus. In Procopius (fl. 495–565) we are confronted with a very different sort of personage, yet one recognized as chief authority for the events of the reign of Justinian. His position in literature is defined by Hodgkin, in his 'Italy and her Invaders,' in following terms:

> After so many generations of decline, here, at length, the intellect of Hellas produces a historian, who, though not equal doubtless to her greatest names, would certainly have been greeted by Herodotus and Thucydides as a true brother of their craft. Procopius has a very clear idea of how history ought to be written. Each of his books, on the Persian, the Vandal, and the Gothic wars, is a work of art, symmetrical, well proportioned, and with a distinct unity of subject. His style is dignified but not pompous, his narrative vivid, his language pure. . . . He exhibits a considerable amount of learning, but without pedantry: and resembles Herodotus in his eager, almost child-like interest in the strange customs and uncouth religions of barbarian nations.

Such appears to be a conservative estimate of Procopius the Cæsarean. He has transmitted to us a vivid pen picture of Vesuvius as observed by him during a four months' sojourn at Naples in 537, at which time an eruption was threatened, though none actually occurred until nearly a century and a half thereafter. Thus it happens that the value of Procopius lies in his excellent topographic description (*de Bello Gothico,* ii., 4), together with hearsay accounts of the two preceding disturbances (*ibid.,* iii., 35). One of the features of the last which we gather from him is that ashes were carried as far as Tripoli. Lava flows are distinctly mentioned both by him and by Cassiodorus as an accompaniment of these eruptions, a fact often overlooked by modern geologists. The Byzantine historian will have further claim to our attention later on.

Following close upon the fall of the Gothic kingdom came the Lombard invasion, which marks the most ill-starred period of Italian history. But little direct and contemporary testimony to historical facts has come down to us from the Lombards, but as their rule approached its end, a native historian arose who preserves the memory of foreign mastery, and ranks as the most distinguished writer of this early part of the middle ages in Italy. This historian is Paul the Deacon (720–*c.* 787), to whom we are chiefly indebted for a history of the Lombards and a revision of Eutropius. Both of these writings contain mention of Vesuvian eruptions, and it is interesting to note that we find in them the earliest suggestion that the original Somma crater had been shattered by the Plinian catastrophe.[3] Paulus Diaconus, in his 'Historia Langobardorum,' and the Roman 'Liber Pontificalis,' a compilation due to many hands and extending over a number of cen-

[3] 'Hist. Lang.,' edited by Muratori, *R. I. S.*, Vol. V., p. 59.

turies, are our only sources relating to the ash-eruption of 685, which continued 'mense martio per dies aliquot.' This is reckoned as the fifth in the list of recorded disturbances.

There now ensues a period of dire poverty in Italian chronicles, and we hear nothing further of Vesuvius for nearly three hundred years. Then are found meager and incidental notices of two eruptions, during the earlier of which lava flows reached the sea, a symptom of high intensity, only twice repeated in modern times. The unique authority for these events is Petrus Damianus, a prolific and most singular polemical writer of the early eleventh century, monk and cardinal, whom Balzani describes as having 'treated in prose and verse every possible subject, whether in literature, homilies, lives of saints, political or religious treatises.[4] Some confusion exists regarding the dates of these eruptions. This arises from the fact that the years in which they occurred are unmentioned, although names are given instead of petty princes with whose deaths they synchronized, the coincidence being interpreted in a manner usual to the times. Later on we find an extract of Peter's account appearing in a postscript to Leo of Marsi's 'Chronicon' under date of 1049, where, unfortunately, the name of the reigning duke of Naples, John III., is omitted, thus leaving it uncertain which one of the numerous family of Capuan princes was intended. Recently the tangle has been unraveled by the Neapolitan historian Capasso,[5] who is no doubt correct in assigning the events in question to the years 968 (in lieu of 982) and autumn of 999, respectively. By a fortunate chance, the original draft of Leo of Marsi's chronicle is still preserved at Munich, and, with its erasures and numerous additions, clearly shows what use was made by the author of his materials in preparing finished copy.

For a brief mention of the eighth recorded eruption we are indebted to a wandering monk, Rodulphus Glaber, of whom little is known except that he lived at various monasteries, including those of Bèze and Cluny, after having traveled extensively in Italy. His history, published 1047, is not without value for contemporary events, and is regarded as reliable in the main, hence no reason appears for doubting his account of a violent eruption in 1007. By some authors the passage has been understood to read seven years *before* instead of after the millenium, hence the earlier date is often incorrectly given in catalogues of eruptions. Mabillon recalls that the year 1006 was

[4] Ugo Balzani, 'Le cronache italiane nel medio evo.,' 2d ed. (Milan, 1900). The complete works of Petrus Damianus are edited by Migne, 'Patrolog. lat.,' Vols. CXLIV., CXLV. (Paris, 1853). Cf. Opusculis xix., c. 9 et 10.

[5] B. Capasso, 'Monumenta Neapolitani Ducatus,' Vol. I., p. 114. For an account of the Pandulf line of princes, see the article by M. Schipa in *Archiv. Storico Prov. Napoletane*, ann. XII. (1887), p. 254, and compare the genealogical table given in Pflugk-Harttung's 'Iter Italicum,' p. 711.

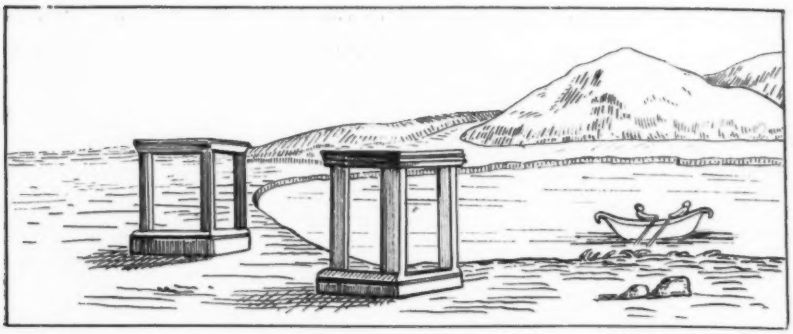

FIG. 1. VESUVIUS AS REPRESENTED IN ROMAN TIMES. From facsimile engraving of a Pompeian fresco in 'Pitture d' Ercolano e dintorni.'

memorable for a number of calamitous happenings, and suggests that the injury wrought by Vesuvius of which Rodulphus informs us may have fallen properly about that time. Bombs were projected on that occasion to a distance of three miles, issuing from a greater number of orifices than usual, and the noxious gases accompanying the eruption rendered the country round about uninhabitable.[6]

An interval of thirty years ensued before the ninth eruption (1037), which was succeeded by a little over a century's repose. After 1139 no further disturbance is known to have taken place until early in the sixteenth century, although Mount Epomeo, in the neighboring island of Ischia, was active in the year 1302. Brief records of the ninth and tenth eruptions are found in monastic chronicles, compiled in near-by abbeys, and of noteworthy importance. Foremost should be mentioned the monastic histories written during the eleventh century at Monte Cassino, a famous abbey of ancient foundation and mother of all Benedictine monasteries, which shone like a light in the dark ages. Of

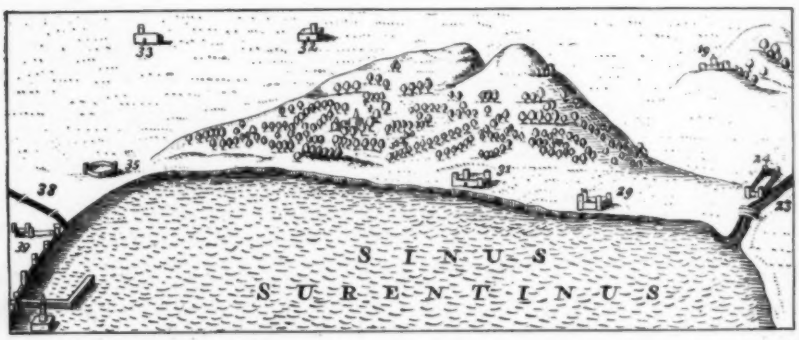

FIG. 2. VESUVIUS ABOUT THE YEAR 1500. From the earliest known engraving of the mountain and surroundings, printed 1514. 19, Palma; 23, Sarnus fl.; 24, Pompeii; 29, Turre Nunciatæ; 31, Herculaneum; 32, Marilianum; 33, Pomilianum; 35, Palæpolis; 38, Sebethus fl.; 39, Neapolis.

[6] *M. G. H., SS.*, Vol. VII., p. 61.

interest for our present purpose are the 'Annales Casinenses' (1000–1212),[7] a compilation by various monks whose names are unknown; the 'Chronicon Casinensis'[8] of Leo of Marsi, better known, after becoming

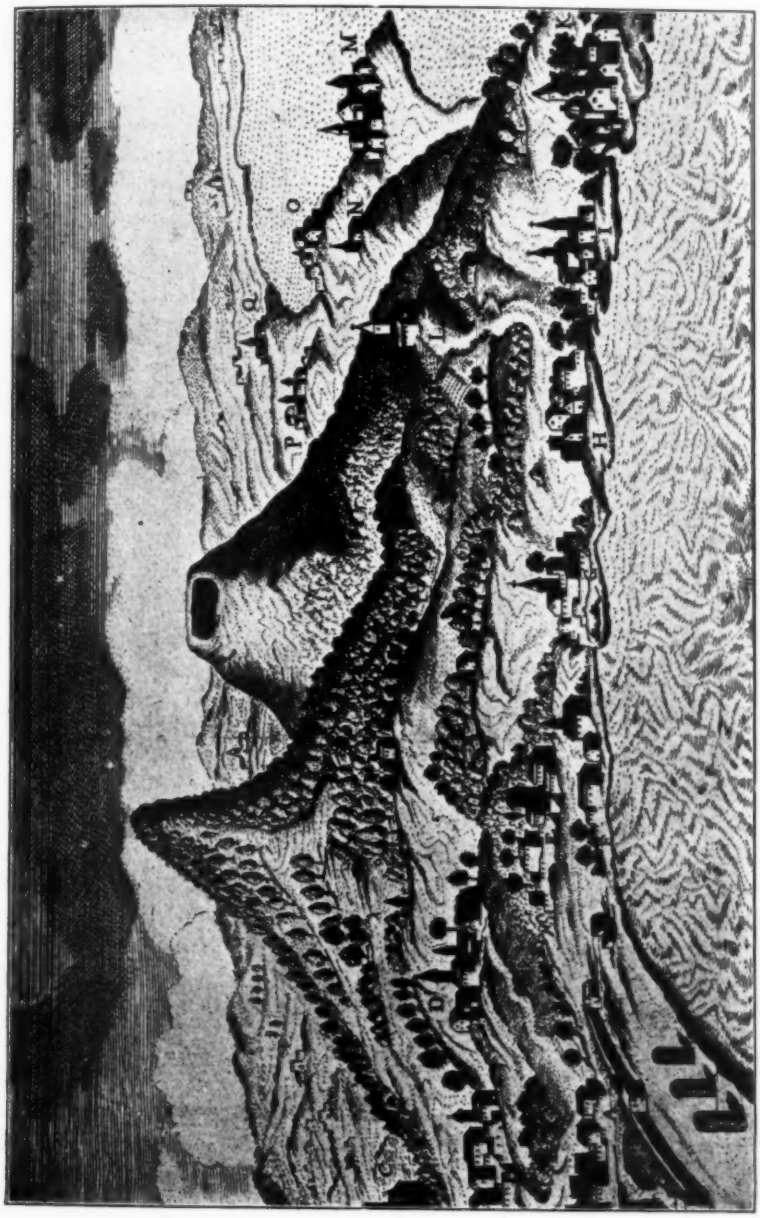

FIG. 3 ATTEMPTED RESTORATION OF VESUVIUS PRIOR TO THE ERUPTION OF 1631. From Mecatti, after an early print. *A*, Barra; *B*, Massa di Somma; *C*, Maria dell' Arco; *D*, S. Sebastino; *E*, S. Giovanni a Teduccio; *F*, S. Maria del Soccorso; *G*, Pietra Bianca; *H*, Portici; *I*, Resina; *K*, Torre del Greco; *L*, S. Maria a Pugliano; *M*, Torre dell' Annunziata; *N*, Camaldoli della Torre; *O*, Torre Scassata; *P*, Boscotrecase; *Q*, Sarno.

[7] *M. G. H., SS.*, Vol. XIX., pp. 306, 309.
[8] *Ibid.*, Vol. VII., pp. 670, 684.

Bishop of Ostia, as Leo Ostiensis, one of the most sober and important of Italian historians; the 'Annales Cavenses' (569–1318),[9] produced by another famous monastery near Salerno; and finally the 'Chronicon' (1102–1140)[10] of Falco of Benevento, notary, judge and papal chancellor, to whom posterity is indebted for precious information. These contemporary sources contain all that is known of the ninth and tenth Vesuvian eruptions. Details are wanting, but it is said of the former that it happened in January, 1037, and lava flows reached the sea; the duration of the latter (1139) is stated in one account to have been eight, in another, forty days. Critical estimates of the documents above referred to will be found in various works dealing with the sources of medieval history, amongst which it will be sufficient to mention an article by Hirsch on 'Desiderius of Monte Cassino.'[11] Our review of the chronology of eruptions in the early middle ages is now completed.

There remains to be considered a question that has often been asked, and variously answered: was the form of Somma-Vesuvius essentially the same in antiquity as we know it to-day, or were the ancients acquainted with only a single crateriform summit whose broken wall now partially encircles the newer cone? The only reason for raising the inquiry at all is that neither by direct statement nor by implication do any of the ancient authors allude to Vesuvius as a double-peaked mountain, and the older topographic descriptions can with difficulty be reconciled with the present form of the volcano. It appears indeed passing strange that Strabo, Pliny, Cassius Dio and Procopius should all have remained silent respecting the most salient feature of Vesuvius as viewed from the west, in case its twin peaks presented to their eyes, as they do to ours, almost identical outlines. Yet, accepting their accounts at face value, the only conclusion possible is that the younger cone has been entirely built up during the middle ages, a far shorter interval than is demanded by geologic evidence. A time allowance of barely a thousand years (or at the most fifteen hundred, if we leave Procopius out of the reckoning and admit the correctness of Leone di Ambrogio's figure of a double summit in 1514) for the formation of the central cone is absurdly inadequate, the number of eruptions contributing towards it too few, and their intensity too slight, to have performed the work. This we know from the present slow rate of accumulation, and from the relatively unimportant changes wrought by even paroxysmal eruptions. And it may well be doubted whether the convulsion of 79 A. D. was of more violent character than those of 1631 and 1906, these three exceeding all others in intensity.

[9] *Ibid.*, Vol. III., p. 189.
[10] Muratori, *R. I. S.*, Vol. V., p. 128.
[11] *Forsch. deutsch. Gesch.*, Vol. VII., 1867, pp. 1–112.

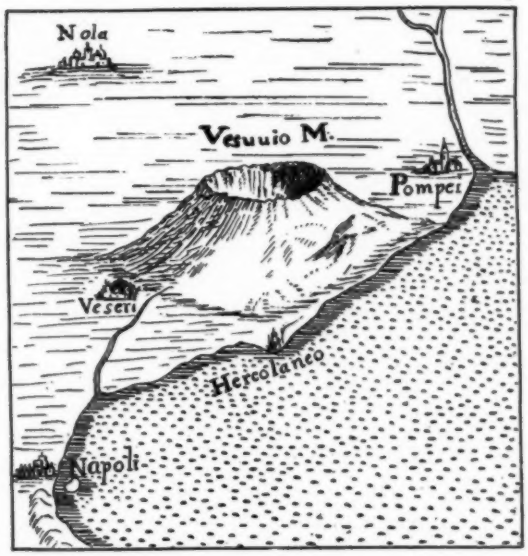

FIG. 4. ATTEMPTED RESTORATION OF THE PARTHENOPIAN VOLCANO AS IT APPEARED IN STRABO'S TIME. After Pellegrino, 1651. (The site of Veseri is conjectural.)

The problem of the geologist is to determine the past condition of things from what he is able to find out from the present. Nevertheless, the tendency of popular opinion has been to subordinate geologic to documentary evidence, and the majority of standard works continue to uphold the view that Vesuvius proper was non-existent at the time Herculaneum and Pompeii were overwhelmed. As positive a statement of this view as any is the following, from Professor Phillips' excellent work on 'Vesuvius':

> Somma, the broken crest of a greater and earlier volcanic crater, has been unmoved in place, unchanged in form and height, through eighteen centuries; a grand and awful fragment left after the poetic 'struggle of earth and sky,' and full of peculiar records of the combat. Vesuvius, born of Somma, and seated within the encircling grasp of its parent, is a variable heap thrown up from time to time, and again, not seldom, by a greater effort of the same force, tossed away into air. . . . Thus two classes of forms arise in the history of Vesuvius: one may be called the old or Somma form, left after violent and exhaustive efforts of the volcano; the other the new form, in which Vesuvius takes a place unrecorded in ancient history (p. 174).

Equally confident is the tone assumed by Professor Judd, in his volume on 'Volcanoes,' in the International Science Series:

> Nothing is more certain than the fact that the Vesuvius upon which the ancient Romans and the Greek settlers of southern Italy looked, was a mountain differing entirely in its form and appearance from that with which we are familiar. The Vesuvius known to the ancients was a great truncated cone, having a diameter at its base of eight or nine miles, and a height of about

4,000 feet. The summit of this mountain was formed by a circular depressed plain, nearly three miles in diameter, within which the gladiator Spartacus, with his followers, were besieged by a Roman army (p. 83).

The above description is reinforced by a figure of a truncate colossus, supposed to represent Vesuvius in the time of Strabo, a graphic portrayal that has been popular ever since the first attempt in this direction was essayed by Camillo Peregrino,[12] in 1651. Strange as it may seem, some writers have been misled into supposing that such was the actual form presented by the mountain in the middle of the seventeenth century. As a matter of fact, all these fanciful restorations of the Somma form of crater, however cleverly they may interpret geological evidence, and to that extent suggest remote prehistoric conditions, can not be considered as having any real historical foundation. For we have no right to interpret literary documents in a manner wholly discordant with what is known of the structure and behavior of the mountain itself, but rather should first seek to establish their credibility by scrutinizing them in the light of ascertained facts. If it has been easy to misconstrue Braccini's account of the crater in the first quarter of the seventeenth century, should we not be wary of accepting the usual rendering of ancient authors? And who is so bold as to claim that the huge truncate cone of which Strabo is commonly understood to speak finds any visible support in Pompeiian wall frescoes, of which several representing Vesuvius in a more or less idealized fashion have been discovered? Impressionistic as all of these paintings are, it is not difficult to perceive that the local scene which caught the artist's fancy was after all not very different from that which still meets our gaze from within or hard by the disinterred city.[13]

We may affirm, then, this conclusion: there is no good reason to suppose that Vesuvius appeared materially different in the yesterday of one or two thousand years ago than it does to-day. The summit of the younger cone, still partially encircled by the ancient Somma rim, has been undergoing comparatively slight modification throughout probably the whole course of human history. And we must perforce believe it to have been existent even before the race of man had appeared on the face of the earth, and had begun to acquire dominion over it.

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[12] 'Discorsi della Campania Felice,' p. 309. (Naples, 1651.)

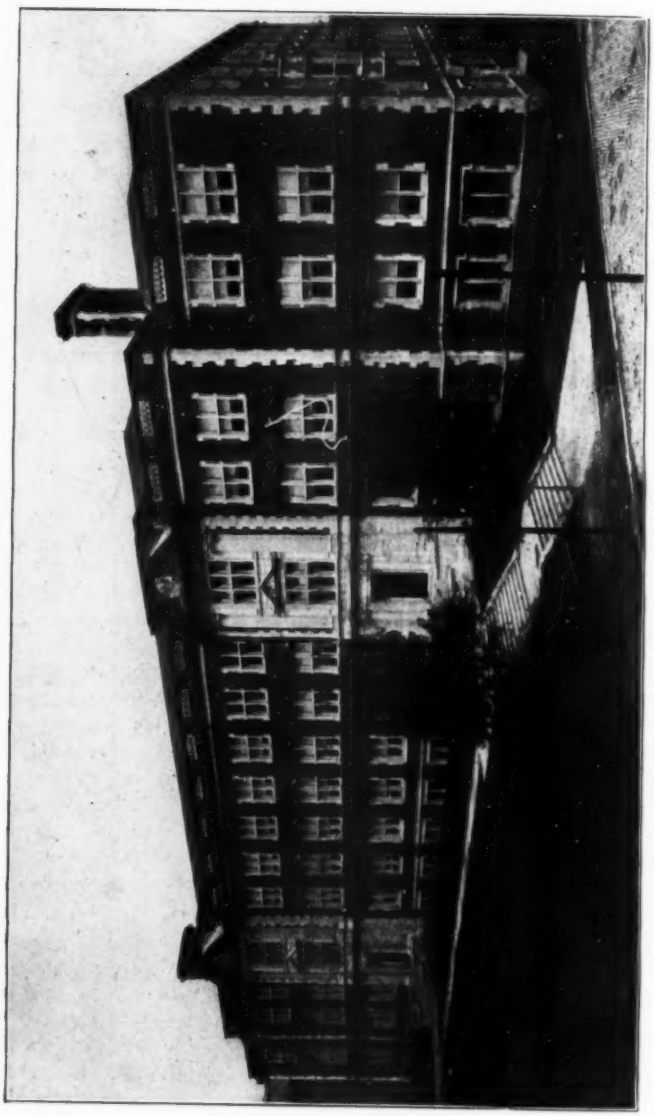
[13] For a recent and interesting discussion of this whole matter, and also of the events of the Plinian eruption, one may consult the following: Enrico Cocchia, 'La forma del Vesuvio,' an essay reprinted in Volume III. of his 'Saggi Filologici' (Naples, 1902); and S. Herrlich, 'Die antike Ueberlieferung über den Vesuv-Ausbruch im Jahre 79.' *Beitr. zur alten Gesch.*, Vol. IV. (1904), pp. 209–226.

THE PROGRESS OF SCIENCE.

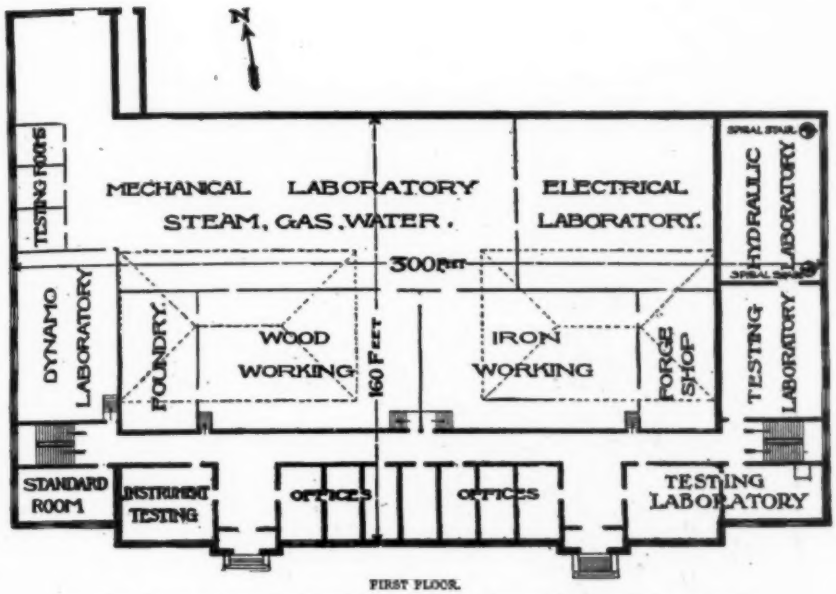
THE NEW ENGINEERING BUILDING OF THE UNIVERSITY OF PENNSYLVANIA

THE new engineering building of the University of Pennsylvania was dedicated on October 19, in the presence of delegates from over one hundred scientific institutions and societies and representatives of six leading foreign nations. The building was open for inspection in the morning, and after luncheon had been served in the building the formal ceremonies took place. Provost Harrison accepted the building on behalf of the trustees, thanking especially Professors Spangler and Marburg, the heads of the departments of mechanical, electrical and civil engineering that occupy the building, the architects, Messrs. Cope and Stewardson, the workmen and the numerous donors who had made the building possible. The degree of doctor of science was conferred on a number of eminent engineers, and the principal addresses were made by Mr. Frederick W. Taylor, the president of the American Society of Mechanical Engineers, and Dr. Alexander C. Humphreys, president of the Stevens Institute of Technology.

The building, a view of which and a general plan of the first floor are shown in the accompanying illustrations, is the largest of the seventy buildings now occupied by the University of Pennsylvania, having a frontage of 300 feet and a depth of 210 feet. The cost, including equipment, was almost one million dollars. It is of fire-proof construction, and the equipment is of the most modern and approved type. The exterior is of dark brick, with limestone trimmings, and the general architectural treatment is in the English-Georgian school, in accord with the later university halls. There are three stories, the total floor area being 128,000 square feet. The heating is by direct steam; the ventilation by electrically-driven fans, and the lighting by electricity. The steam for the engines is supplied from the central station of the university, and, after being used by the engines, is sent into the heating system of the building. There are two principal entrances leading to the main hallway, which extends east and west the entire length of the building to staircases at both extremities. The basement contains locker rooms, lavatories, machinery for heating and ventilating, storage battery rooms, laboratories for geodetic and hydraulic work, and for the testing of the materials of construction. On the first floor, adjacent to the main entrance, are the offices of the heads of departments, the eastern part of the building being devoted entirely to the civil engineering department, and the western part to the mechanical engineering department. Accommodation is also provided for physical and hydraulic testing, instrument testing and for special work in mechanical and electrical engineering. Rooms are likewise set aside for dynamos and electric motors, steam and gas engines, refrigerating apparatus, hydraulic motors, boiler testing, pattern-making, wood and iron working, foundry and machine shops, etc. On the second floor is a reference library and reading room, a students' assembly room, rooms for the use of instructors and for lectures and recitations. The rear portion of this floor is devoted almost wholly to drawing rooms. A room for the use of the engineering societies, a general supply store, and the library stack oc-



THE ENGINEERING BUILDING OF THE UNIVERSITY OF PENNSYLVANIA



GROUND PLAN OF THE ENGINEERING BUILDING, UNIVERSITY OF PENNSYLVANIA

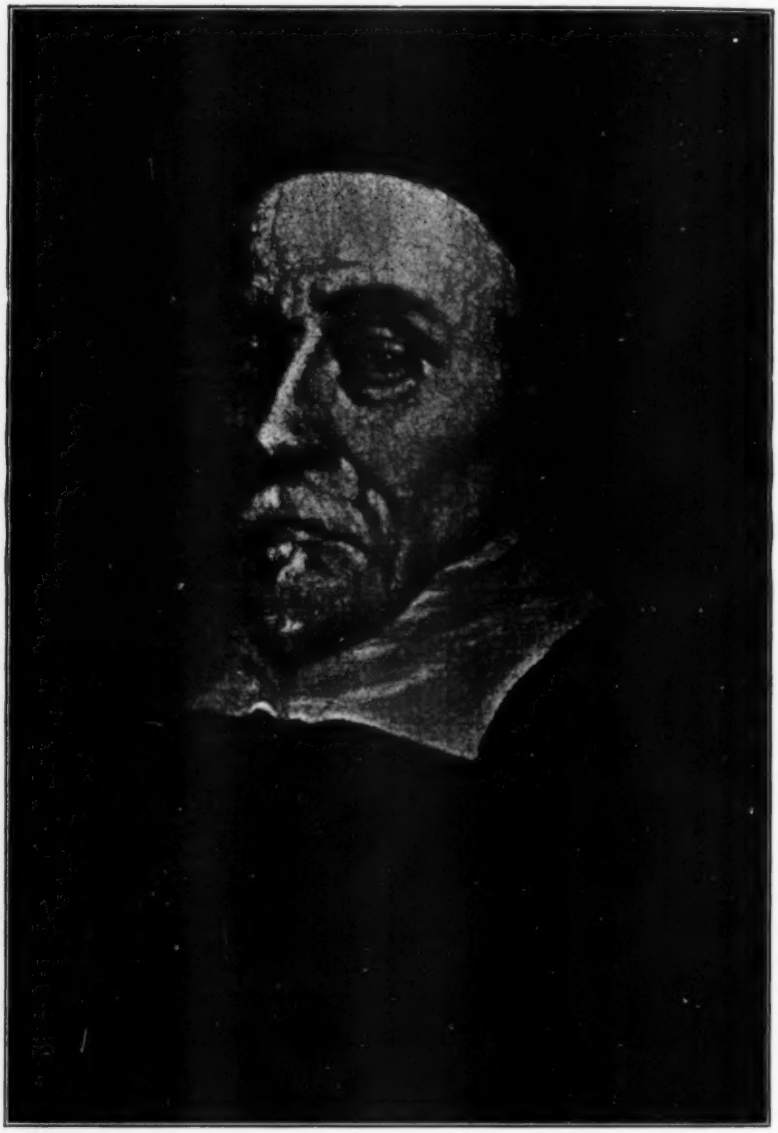
cupy the middle portion. In the east and west wings ample space is assigned to the engineering museums, while the rear of this floor is set aside exclusively for additional drawing rooms, which, like those just beneath, will have the full advantage of a north light.

The engineering department of the university was established in 1874, but the constant increase of numbers in the classes of the departments has necessitated their moving into more spacious quarters three times since their founding. The departments this year have a total enrollment of nearly six hundred students, and a teaching force of forty.

As Mr. Taylor said at the close of his address: "Philadelphia is the center of the largest and most diversified group of engineering and manufacturing enterprises in this country. The engineering schools of the University of Pennsylvania already stand high; but it seems to me that the opportunity lies open to them even more than to their famous medical and law schools to stand at the very top. This magnificent building, equipped as it is with the latest and best of everything, is the first and a great step towards this end. But after all, your largest possibility and one which does not exist for, and can not be created by, any other American university, lies in the opportunity for bringing your students into close touch and personal contact with the men who are working in and managing the great industrial establishments of Philadelphia."

THE HARVEIAN ORATION

THE Harveian oration, delivered annually before the Royal College of Physicians, London, was given on October 18 by Dr. William Osler, regius professor of medicine in the University of Oxford, and formerly professor of medicine in the Johns Hopkins University, who chose as his subject 'The Growth of Truth: as illustrated in the discovery of the circulation of the blood.' With his felicity of expression and wealth of knowledge of the history of medicine and science, Dr. Osler reviewed again the instructive story of the discovery. Though rehearsed now for two hundred and fifty years by Harveian orators, the story loses in Dr. Osler's



WILLIAM HARVEY.

From a painting by De Reyn, in the rooms of the Royal Society; reproduced in Sir William Huggins's 'The Royal Society.'

hands none of its interest and is well worth the repetition.

By the terms of Harvey's bequest, founding the lectureship, the orator is among other things enjoined to exhort the fellows of the college 'to search out and study the secrets of nature by way of experiment.' The discovery of the circulation of the blood is itself a conspicuous illustration of the fruitful application of the experimental method to a fundamental physiological problem, as well as a type of the evolution of scientific truth through the various stages, from the accumulation of facts and observations to a universally accepted explanation. The discovery of the general circulation is one of the important landmarks in the history of science. It evidences, on the one hand, in a peculiarly striking manner, the overthrow of authority and the birth of the modern scientific spirit. On the other hand, in the discovery and more particularly in the methods by which it was attained are laid the foundations of experimental physiology and medicine, which have contributed so incalculably to human welfare.

The sixteenth century had witnessed notable contributions to the knowledge of the structure of the human body. The work of Vesalius, Fallopius and Fabricius had brought about numerous corrections in the anatomy of Galen, which had been taught in the schools with but little change for fourteen centuries. The Galenical physiology remained, however, in complete authority on all matters pertaining to the functions of the body. It taught that the liver is the source of the blood, one kind of which flowed to the right ventricle and thence by way of the veins to the lungs and the general system, the other by way of the arteries from the left ventricle to the lungs and general system. It was supposed that the blood flowed directly from the right to the left ventricle through the pervious septum of the heart. The muscular function of the heart in propelling the blood was unknown. Such were the prevailing conceptions in Harvey's time. While the lesser circulation had been fully described, its function was supposed to be the nutrition of the lungs. It may be doubted whether any one had even glimmerings of the greater or systemic circulation. The demonstration of this was Harvey's great work, all the more notable since the estimation of the significance of a scientific discovery is determined by the state of knowledge at the time when it is made.

After studying medicine at Padua under Fabricius, the foremost anatomist of the day, Harvey returned to London as a general practitioner, and in the course of time was appointed Lumleian lecturer to the Royal College of Physicians. It was before the president, censors and fellows of the college and an unusually large company of laymen that, on April 17, 1616, he demonstrated the structure and action of the heart and propounded his new doctrine of the general circulation. The formal announcement of the discovery in the *De Motu Cordis et Sanguinis* was, however, deferred for twelve years, furnishing an interesting instance of delay in the publication of a great discovery through fear of the prejudices of men. The motive which actuated Harvey is not far to seek. So complete was the dominance of the authority of Hippocrates, Galen and Columbus that few would have had the temerity to question doctrines which had the sanction of the ancients and were sealed by general acceptance.

The new theory was as usual given a tardy recognition, and in some parts, notably in France, was met with open hostility. Owing, however, to the care and accuracy in observation and experimentation, to the fullness of the array of evidence adduced, and to the brilliancy of the demonstration, its truth could not long be denied. He himself lived to see his discovery all but universally accepted, and Hobbes's oft-quoted statement is well-nigh literally true that Harvey, 'conquering envy,

hath established a new doctrine in his lifetime.' Complete and universal acceptance was, however, not obtained until late in the century, even after Malpighi had in 1661 by means of the newly invented microscope completed the demonstration of the circulation by tracing the course of the blood in the capillaries. In contrast with the tenacity with which traditional conceptions have been adhered to in the past, as illustrated in this discovery and its reception, and the whole history of science is strewn with more bitter struggles for acceptance, it is gratifying to note the receptivity of the public mind to new theories in our day and the tolerance and readiness with which they are accorded a hearing.

The significance of Harvey's work lay not merely in the discovery of the circulation of the blood and the profound changes which it wrought in the conceptions of the functions of the body and in medical thought and practise, great though these were. It lay more perhaps in its contribution to the development of scientific method. In the words of the Harveian orator, "here for the first time a great physiological problem was approached from the experimental side by a man with a modern scientific mind who could weigh evidence and not go beyond it, and who had the sense to let the conclusions emerge naturally but firmly from the observations. To the age of the hearer, in which men had heard, and heard only, had succeeded the age of the eye, in which men had seen, and had been content only to see. But at last came the age of the hand—the thinking, devising, planning hand; the hand as an instrument of mind now reintroduced into the world in a modest little monograph of seventy-two pages, from which we may date the beginning of experimental medicine."

SCIENTIFIC ITEMS

SIR DAVID GILL, astronomer royal in South Africa, has retired.—Dr. Wilhelm Waldeyer, professor of anatomy at Berlin, and secretary of the Berlin Academy of Sciences, has celebrated his seventieth birthday.—The sum of about $5,000 has been subscribed for the foundation for the advancement of geographical study in honor of Dr. von Neumayer.—Advantage will be taken of the twentieth anniversary of the isolation of fluorine to present a gold medal to M. Moissan.—In acknowledgment of his work in metallurgical research, Professor Henry M. Howe, of Columbia University, has received from the Russian emperor the order of the Knighthood of St. Stanislas.

DR. SVEN HEDIN, who by order of the government was denied access to Tibet from the side of India, is making good his entry into western Tibet from Chinese Turkestan.—Mr. Walter Wellman and Major Hersey have returned to this country. The former will go to Paris in six weeks to continue his supervision of the changes in his airship. Major Hersey will accompany the Chicago *Record-Herald* expedition in its attempt to reach the Pole next summer.—Captain Roald Amundsen sailed on November 8, on the Scandinavian-American steamer *Hellig Olaf*, for Christiania, where the records of his magnetic observations in the Arctic will be worked out. Captain Amundsen has presented his entire collection to the Norwegian government. The new king of Norway has conferred upon him the highest decoration of the kingdom, the grand cross and cordon of St. Olaf.

A SCIENTIFIC session of the National Academy of Sciences was held at the Harvard Medical School, Boston, beginning on Tuesday, November 20.—The American Association for the Advancement of Science and the twenty or more national scientific societies affiliated with it will meet in New York City during convocation week, beginning on December 27. It is expected that this will be the largest and most important meeting of scientific men ever held in America.

INDEX

NAMES OF CONTRIBUTORS ARE PRINTED IN SMALL CAPITALS

Adams, Mr., on the American College, 94
African Pygmies, S. P. VERNER, 471
Agriculture, Appropriations for the Department of, 286
ALLEN, ROBERT McD., Pure Food Legislation, 52
America and Seismological Research, WILLIAM HERBERT HOBBS, 226
American Association for the Advancement of Science, 89; Cornell University and the Summer Meeting of the, 187
American, Medical Association, the Boston Meeting of the, 91; College, Mr. Adams on the, 94
Americanisms versus Briticisms, EDWIN W. BOWEN, 324
Animal Psychology, Fact and Fable in, JOSEPH JASTROW, 138
Appropriations for the Department of Agriculture, 286
ASHLEY, GEORGE H., The Geological Prelude to the San Francisco Earthquake, 69
Atlantic Coast, Waterway Defenses of, WILLIAM J. ROE, 530
Atoms, Are they Divisible, are the Elements Transmutable and Forms of Matter but Modes of Motion? S. LAWRENCE BIGELOW, 38

B., M., Sigma Xi, 281
Bacon, Francis, and the Modern University, EDWARD O. SISSON, 320
BAILEY, SOLON I., The Study of Variable Stars, 175
Basin, Alluvial, of the Mississippi, The Protection of, ROBERT MARSHALL BROWN, 248
BAUER, L. A., Seismograph and Magnetograph Records of the San Francisco Earthquake, 116
Beit, Alfred, The Will of, 287
BICKNELL, PERCY F., John Stuart Mill, 451
BIGELOW, S. LAWRENCE, Are the Elements Transmutable, the Atoms Divisible and Forms of Matter but Modes of Motion?, 38
Birth Rate, SIDNEY WEBB, 512
Bogoslofs, The, DAVID STARR JORDAN and GEORGE ARCHIBALD CLARK, 481
Boston Meeting of the American Medical Association, 91
BOWEN, EDWIN W., Briticisms versus Americanisms, 324
Boys, Difficult, J. MADISON TAYLOR, 338
Briticisms versus Americanisms, EDWIN W. BOWEN, 324
British, Medical Association and the University of Toronto, 381; Association, York Meeting of the, 479
BROWN, ROBERT MARSHALL, Protection of the Alluvial Basin of the Mississippi, 248
Burbank, Luther, Scientific Aspects of Work of, VERNON L. KELLOGG, 363

CALKINS, GARY N., Pathogenic Protozoa, 409
Canyon, Grand, of the Colorado, The Making of, A. R. CROOK, 417
Carbons and Diamonds in Brazil, H. W. FURNISS, 272
CLARK, GEORGE ARCHIBALD, and DAVID STARR JORDAN, The Bogoslofs, 481
Climate, Changes of, ROBERT DEC. WARD, 458
Coal-Tar Color Industry, the Jubilee of, and Sir William Henry Perkin, 475
College, The American, Mr. Adams on the, 94
Colorado, Grand Canyon of the, The Making of, A. R. CROOK, 417
Control, University, J. J. STEVENSON, 385
Cornell University and the Summer Meeting of the American Association, 187
Correspondence and Discussion, 375
CROOK, A. R., The Making of the Grand Canyon of the Colorado, 417

DALL, WILLIAM HEALEY, Reminiscences of Yukon Exploration, 128
Death, the Causes of, 95
Defenses, Waterway, of the Atlantic Coast, WILLIAM J. ROE, 529
Degeneracy, Physical, or Race Suicide?, SIDNEY WEBB, 512
DE LAND, FRED, Notes on the Development of the Telephone Service, 45, 425, 490
Diamonds and Carbons in Brazil, H. W. FURNISS, 272
Difficult Boys, J. MADISON TAYLOR, 338
Discussion and Correspondence, 375

Earthquake, The San Francisco, The Geological Prelude to the, GEORGE H. ASHLEY, 69; The Investigation of, G. K. GILBERT, 97; Seismograph and Magnetograph Records of, L. A.

BAUER, 116; Rift of 1906, President DAVID STARR JORDAN, 289
Earthquakes, our Greatest, MYRON LESLIE FULLER, 76
EASTMAN, CHARLES R., Vesuvius in the Early Middle Ages, 558
ECKHARDT, C. C., The World State, 147
Education, The Bureau of, 283
Elements, The, Are they Transmutable, the Atoms Divisible and Forms of Matter but Modes of Motion?, S. LAWRENCE BIGELOW, 38
Embryology, The Relations of, to Medical Progress, CHARLES SEDGWICK MINOT, 5
Engineering Building of the University of Pennsylvania, 567
Environment and Race, The Jews, a Study of, MAURICE FISHBERG, 257, 441, 502
Epidemic of Typhoid Fever at Palo Alto, EDITH V. MATZKE, 87
Ether, The Plane of, DAVID STARR JORDAN, 29
Exploration, Yukon, Reminiscences of, WILLIAM HEALEY DALL, 128
EASTMAN, CHARLES R., Vesuvius in the Early Middle Ages, 557

FISHBERG, MAURICE, The Jews, a Study of Race Environment, 257, 441, 502
FLEXNER, SIMON, Immunity in Tuberculosis, 229
Flowers, Wooden, ORVILLE PAUL PHILLIPS, 65
Food, Pure, Legislation, ROBERT MCD. ALLEN, 52; National Law, 385
French Spelling, The Simplification of, BRANDER MATTHEWS, 539
FULLER, MYRON LESLIE, Our Greatest Earthquakes, 76
FURNISS, H. W., Diamonds and Carbons in Brazil, 272

Genius, ROBERT MORRIS OGDEN, 352
Geological Prelude to the San Francisco Earthquake, GEORGE H. ASHLEY, 69
GILBERT, G. K., The Investigation of the San Francisco Earthquake, 97
Grand Canyon of the Colorado, The Making of, A. R. CROOK, 417
GRATACAP, L. P., The Largest American Collection of Meteorites, 21
Greek Thought, The Birth of the Idea of Spirit in, MARY MILLS PATRICK, 357
Harvard Medical School, The New, 93
Harveian Oration, 569
HOBBS, WILLIAM HERBERT, America and Seismological Research, 226
HOLDEN, EDWARD S., The Measure of Progress, 155
Homicide in American Cities, the Effects of Immigration on, MAYNARD SHIPLEY, 160; MORRIS LOEB, 375

Immigration, the Effects of, on Homicide in American Cities, MAYNARD SHIPLEY, 160; MORRIS LOEB, 375
Immunity in Tuberculosis, SIMON FLEXNER, 229

JASTROW, JOSEPH, Fact and Fable in Animal Psychology, 138
Jews, The, A Study of Race and Environment, MAURICE FISHBERG, 257, 441, 502
JORDAN, DAVID STARR, Plane of Ether, 29; The Earthquake Rift of 1906, 289; and GEORGE ARCHIBALD CLARK, The Bogoslofs, 481
Jubilee of the Coal-Tar Color Industry and Sir William Henry Perkin, 475

KELLOGG, VERNON L., The Scientific Aspects of Luther Burbank's Work, 363

Legislation, Pure Food, ROBERT MCD. ALLEN, 52; and Science, 190
LOEB, MORRIS, The Effects of Immigration on Homicide in American Cities, 160

MACDOUGAL, D. T., Discontinuous Variation in Pedigree-cultures, 207

Magnetograph and Seismograph Records of the San Francisco Earthquake, L. A. BAUER, 116
MATTHEWS, BRANDER, The Simplification of French Spelling, 539
MATZKE, EDITH V., The Typhoid Fever Epidemic at Palo Alto, 87
Measure of Progress, EDWARD S. HOLDEN, 155
Mechanics, The Development of, S. E. SLOCUM, 268
Medical, Progress, the Relations of Embryology to, CHARLES SEDGWICK MINOT, 5; Association, American, the Boston Meeting of the, 91; British, and the University of Toronto, 381; School, the New Harvard, 93
Mendeleeff, Professor, 285
Meteorites, The Largest American Collection of, L. P. GRATACAP, 21
Middle Ages, Vesuvius in the, CHARLES R. EASTMAN, 558
Mill, John Stuart, PERCY F. BICKNELL, 451
MINOT, CHARLES SEDGWICK, The Relations of Embryology to Medical Progress, 5
Mississippi, Protection of the Alluvial Basin of the, ROBERT MARSHALL BROWN, 248

OGDEN, ROBERT MORRIS, Genius, 352
Osler's Harveian Oration, 569

Pathogenic Protozoa, GARY N. CALKINS, 409

PATRICK, MARY MILLS, The Birth of the Idea of Spirit in Greek Thought, 357
Pedigree-cultures, Discontinuous Variation in, D. T. MACDOUGAL, 207
Pennsylvania, University of, Dedication of the New Engineering Buildings, 567
Perkin, Sir William Henry, and the Jubilee of the Coal-Tar Color Industry, 475
PHILLIPS, ORVILLE PAUL, Wooden Flowers, 65
Physical Degeneracy or Race Suicide?, SIDNEY WEBB, 512
Plane of Ether, DAVID STARR JORDAN, 29
POINCARÉ, H., The Value of Science, 193, 310, 398, 545
Progress of Science, 89, 187, 283, 380, 475, 567; The Measure of, EDWARD S. HOLDEN, 155
Protozoa, Pathogenic, GARY N. CALKINS, 409
Psychology, Animal, Fact and Fable in, JOSEPH JASTROW, 138
Pure Food Legislation, ROBERT MCD. ALLEN, 52; National Law, 385
Pygmies, The African, S. P. VERNER, 471

Race and Environment, The Jews, a Study of, MAURICE FISHBERG, 257, 441, 502; Suicide, or Physical Degeneracy?, SIDNEY WEBB, 512
Research, Seismological, and America, WILLIAM HERBERT HOBBS, 226
Rift, Earthquake, of 1906, DAVID STARR JORDAN, 289
ROE, WILLIAM J., Waterway Defenses of the Atlantic Coast, 530
Royal Society, 476

San Francisco Earthquake, the Geological Prelude to, GEORGE H. ASHLEY, 69; The Investigation of, G. K. GILBERT, 97; Seismograph and Magnetograph Records of the San Francisco Earthquake, L. A. BAUER, 116; Rift of 1906, DAVID STARR JORDAN, 289
Science, The Progress of, 89, 187, 283, 380, 475, 567; The American Association for the Advancement of, 89, 187; and Legislation, 190; the Value of, H. POINCARÉ, 193, 310, 398, 545
Scientific Items, 96, 191, 288, 384, 480, 576
Scientific Aspects of Luther Burbank's Work, VERNON L. KELLOGG, 363
Seismograph and Magnetograph Records of the San Francisco Earthquake, L. A. BAUER, 116
Seismological Research in America, WILLIAM HERBERT HOBBS, 226
SHIPLEY, MAYNARD, The Effects of Immigration on Homicide in American Cities, 160
Shorter Articles, 87, 281
Sigma Xi, M. B., 281
SISSON, EDWARD O., Francis Bacon and the Modern University, 320
SLOCUM, S. E., The Development of Mechanics, 268
Spelling, French, The Simplification of, BRANDER MATTHEWS, 539
Spirit, The Birth of the Idea of, in Greek Thought, MARY MILLS PATRICK, 357
Stars, Variable, The Study of, SOLON I. BAILEY, 175
State, The World, C. C. ECKHARDT, 147

TAYLOR, J. MADISON, Difficult Boys, 338
Telephone Service, Notes on the Development of, FRED DE LAND, 45, 425, 490
Thought, Greek, The Birth of the Idea of Spirit in, MARY MILLS PATRICK, 357
Toronto, University of, and the British Medical Association, 381
Tuberculosis, Immunity in, SIMON FLEXNER, 229
Typhoid Fever Epidemic at Palo Alto, EDITH V. MATZKE, 87

University, Modern, and Francis Bacon, EDWARD O. SISSON, 320; Control, J. J. STEVENSON, 385

Value of Science, H. POINCARÉ, 193, 310, 398, 545
Variable Stars, the Study of, SOLON I. BAILEY, 175
Variation, Discontinuous, in Pedigree-cultures, D T. MACDOUGAL, 207
VERNER, S. P., The African Pygmies, 471
Vesuvius in the Early Middle Ages, CHARLES R. EASTMAN, 558

WARD, ROBERT DEC., Changes of Climate, 458
Waterway Defenses of the Atlantic Coast, WILLIAM J. ROE, 530
WEBB, SIDNEY, Physical Degeneracy or Race Suicide?, 512
Wooden Flowers, ORVILLE PAUL PHILLIPS, 65
World State, C. C. ECKHARDT, 147

York Meeting of the British Association, 479
Yukon Exploration, Reminiscences of, WILLIAM HEALEY DALL, 128

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CONTENTS

The Bogoslofs: President DAVID STARR JORDAN and GEORGE ARCHIBALD CLARK 481

The Development of the Telephone Service. FRED DELAND. 490

The Jews: a Study of Race and Environment. Dr. MAURICE FISHBERG . 502

Physical Degeneracy or Race Suicide? SIDNEY WEBB 512

Waterway Defences of the Atlantic Coast. WILLIAM J. ROE 530

The Simplification of French Spelling. Professor BRANDER MATTHEWS . 539

The Value of Science. Professor H. POINCARÉ 545

Vesuvius in the Early Middle Ages: Dr. CHARLES R. EASTMAN . . . 558

The Progress of Science:

The New Engineering Building of the University of Pennsylvania; The Harvelan Oration, Scientific Items 567

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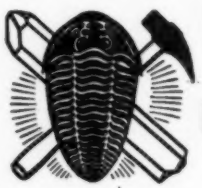

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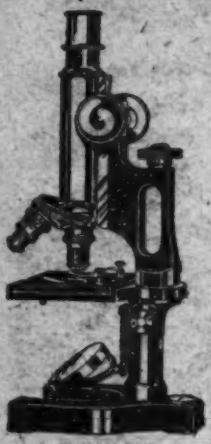

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